

TELLING THE TRUTH IN ECONOMIC THEORY

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AUTHORSHIP

In accordance with the University Regulations, I confirm that the preparation and composition of this thesis is my own work.

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ABSTRACT

Need a theory's assumptions be true? In a since notorious essay, the economist Friedman argued they need not be and in abstract theories often won't be. The first part of the thesis discusses his case, which has been widely misunderstood. It concludes that, whilst false assumptions may fulfil the role of epitomising and implying truth, those in Friedman's key economic example do not. The rest of the thesis then relates Friedman's case to that in defence of general equilibrium theory (the heart of orthodox economic theory) and argues that this defence fails. Two complementary arguments against the defenders' position are presented, the first working from the fact that the theory's assumptions are not true and the second considering what would happen if they were - the conclusion being that the theory relates neither to actual cases nor to possible polar ones. Comparisons drawn with rival economic theories end this case-study in the philosophy of economics.

Chapter 1DESCRIBES A CLOAK

In an oft-quoted passage of his 1938 Address, Harrod says:

"Exposed as a bore, the methodologist cannot take refuge behind a cloak of modesty. On the contrary, he stands forward ready by his own claim to give advice to all and sundry, to criticise the work of others which, whether valuable or not, at least attempts to be constructive; he sets himself up as the final interpreter of the past and dictator of future efforts." "Telling the Truth in Economic Theory" is a philosophical thesis about economic method, that I can only hope will not prove boring. But in this introductory chapter, I am out to defy another part of Harrod's charge,¹ by laying my claim to a cloak.

I want to disclaim, to begin with, having done much more than dipped into much of the literature that is potentially relevant to my general subject: a subject that might be called "the philosophy of economics" (a term with perhaps two advantages over the more familiar "economic methodology": firstly, that of suggesting a more philosophical content; and secondly, that of dodging - if only temporarily - pejorative overtones that the latter term may already be bearing). This subject is comparatively young (setting aside any earlier existences it may have enjoyed); and, at least partly for this reason, its literature poses a dilemma. On the one hand, there is too little: modern works which apply philosophical analysis to questions of economic method are few and far between. On the other hand, there is too much: of what stands

on the library shelves under the general headings, not only of philosophy and of economics themselves, but also of other social sciences (including politics and - on some classifications - history) and of the various natural sciences, a great deal has some potential reference to the philosophy of economics. In this situation, one hardly knows whether to hold to a strict diet, devouring the then meagre stock-in-trade, to face bare shelves and basic principles thereafter; or whether to indulge a more catholic taste, spending years of apprenticeship before even beginning, albeit as a gourmet, to make a start on the central dish. Discovering this dilemma gradually in the course of a basic browse, I have since tried to steer something of a middle course. But inevitably there will be gaps, routes I take that have been tried before and obstacles in the way that, had I but known it, could lightly have been turned by another's hand.

Yet perhaps these very difficulties have made it easier for me to protest against pretending in what follows to "give advice to all and sundry". For some appeal to the principle of division of labour having resulted, at least as much information has been accepted from some fellow labourers, I believe, as advice has been doled out to others; whilst there are some whom I have not been able to deal with at all. I have had no dealings, for instance, with the historians of economic thought; nor any with econometricians as such. And whilst The Grundrisse makes a fleeting appearance later on, I have not entered specifically into the fast-growing Marxist literature either.² On the philosophy of science, as a non-scientist, I remain by and large

agnostic, accepting information on the various developments here and trying to keep in mind, as it were, the possibility space. Then for detailed results in neoclassical economic theory, I am generally glad to go to neoclassical economists themselves, (though I do not go, it must be confessed, quite cap in hand). Likewise for metaphysics, metaphysical writings have been my source. Of advice, there may still be plenty. Perhaps in particular it goes to those who seem to have allowed naive impressions of results in the philosophy of science to colour even their statement of the content of a specific text on economic method. Texts, of course, must be texts; but further, it is not at all clear that conclusions based on a study of natural science, even if correct in that sphere, can legitimately be read (much less misread) over to matters of economic procedure too: simply to do this will surely often be to beg the big question of how far the "dismal" science resembles the natural ones. The approach that I am hostile to for these reasons also seems often to appear as a species of what Ryle has called, in a different context, "facile pigeon-holing". No cloak of modesty here, I admit; but then, I have sought to advise chiefly those who seem already themselves to be giving "advice to all and sundry".

Has the work, then, exposed itself to the charge of being itself only destructive? Well, I do not think so. Again, what is most criticised is, I think, itself somewhat negative doctrine. Then interpretation of the past, there is certainly (though I am not sure I would claim it "final"). But dictation of the future? Rather, I hope, suggestions. One implicit suggestion will be that there are areas of

philosophical analysis that might fruitfully be related to issues in economic method, on a problem-solving basis (a traffic that perhaps need not always be one-way); for it seems to me that a good deal of progress might be made by following the, in some ways modest, approach Emmet and MacIntyre support. They write (1970, p. ix) that they do not hold that "first, one has to select some general philosophical standpoint - such as that of logical empiricism or phenomenology - on epistemological or other philosophical grounds, and then only secondly to apply the methods and insights of the chosen tradition to the problems of the philosophy of social science", continuing that "this seems to us to get things the wrong way round. The value and relevance of any general philosophical standpoint to the philosophy of the social sciences can only be demonstrated by showing the contribution that it makes to the key problems." This allows a good deal of scope to the philosophy of economics; not least because, the subject being so young, there is a backlog of problems to be dealt with. Joan Robinson, for instance reports (1973, p. 122) that "the present state of affairs in theoretical economics is very distressing. There are deep and prolonged controversies going on about purely logical points."

This then is my cloak, in the main of modesty, if enlivened here and there by a patch of stronger stuff.

Appendix IFootnotes to Chapter 1

1. His Address (to Section F of the British Association) was, however, about "The Scope and Method of Economics"; and the passage quoted is just, I think, Harrod's own(attractive) way of disowning a gown of arrogance.
2. In brief encounters with the Marxist literature, lacking previous preparation for it, I have felt some sympathy with Joan Robinson's remark (1973, p. 250) that "you cannot talk to a Marxist in English because he only understands Hegelese, a language I have never mastered."

Chapter 2THE METHODOLOGY MYSTERY

or

THE FRIEDMAN AFFAIR

Lady Jessica: "... if you arrange things not perhaps exactly as they were, but as they ought to have been."

Sir Christopher: "I see. In that way a lie becomes a sort of idealized and essential truth ..."

H.A. Jones, The Liars.

"And, after all, what is a lie? 'Tis but the truth in masquerade."

Byron, Don Juan.

Is lying wrong? That it is was the orthodoxy of our forefathers. But in this permissive age, and more especially since the appearance in 1953 of Friedman's "The Methodology of Positive Economics", most economists are all too aware that time-honoured beliefs are now under review. Need a theory's assumptions be true? Not a bit of it (the Friedman line is supposed to run): all that counts is the truth of the implications - what matters is whether the theory "works". Friedman's message, metaphorically, has seemed to be that good can, or indeed will, come of telling fibs: hence lying is to be tolerated, or even positively praised.

Such heretical sentiments provoked reactions that were indignant,

not to say shocked, on the one hand, and enthusiastic, if tinged with wonder, on the other. There was a trickle of comment, soon swelling to a flood. High-water came in the mid-sixties with the American Economic Association's Symposium on "Problems of Methodology"; but the Symposium discussion of Friedman's case has itself been a source of yet more controversy. The flow of contributions continues still.¹

1974 saw the twenty-first anniversary of the dispute over Friedman's "Methodology".² But its very maturing has posed a fresh problem, little publicised as yet, but yearly becoming both more puzzling and more acute. That problem is: why is it that, so long after his main expression of them became available, no consensus exists about the status of Friedman's views? Evaluating literature meanders on. Meanwhile, some practising economists take the seeming Friedman view as proven and cite it as the rationale of their testing procedures: for example, Crew, Jones-Lee and Rowley state at the outset of a recent paper (1971, p. 173) that "for the most part, the methodology employed in this paper is that established by Milton Friedman where the relevant test of the validity of a theory is a comparison of its predictions with experience"³ (my emphasis). However others - still lamenting with Kaldor, prior to advocating some new assumption⁴, that "unlike any scientific theory, where the basic assumptions are chosen on the basis of direct observation of the phenomena ... the basic assumptions of economic theory are either of a kind that are unverifiable ... or of a kind which are directly

contradicted by observation" (1972, p. 1238)⁵; or still insisting with Leontief, in recommending how the subject should develop, that "it is precisely the empirical validity of [the] assumptions on which the usefulness of the entire exercise depends ... what is really needed is a ... verification of these assumptions in terms of observed facts" (1971, p. 2) - do not cite Friedman's challenge explicitly or discuss criticisms of it⁶, and write not so much as if the issues had been unequivocally settled but rather as if they had never even been raised. Yet Friedman's paper is only some forty pages long; it is written in excellent English prose; and it deals with a fairly well-defined, and on the face of it simple, group of issues. Why then the absence of consensus about his case, despite all the comment over so many years? This is what I call "The Methodology Mystery".

At first, whilst knowing that dispute over "The Methodology" loomed large in the literature on economic method, I imprudently believed that the Mystery of why it did so would not be very hard to solve; and that, once having solved it, one could move briskly on to what then seemed altogether distinct, and more stirring, questions. Having gained my initial impression of what Friedman's argument was from merely hearsay evidence, I unfairly fancied that, in the rather dreary debate, his opponents must simply have failed to push their point right home⁷. But the more closely I studied Friedman's text and read hostile papers criticising it, the less satisfactory the critics' objections appeared to be. It began to be clear that much of the commentary (and even of the favourable commentary) has mis-

represented Friedman's case - that his line does not run as has commonly been supposed; and it became apparent too that issues raised by the argument he himself presents do bear on some of the questions I had earlier taken to be entirely independent ones. And now the next three chapters, in their various ways, all concentrate on the Methodology Mystery.

Chapter 3 treats of a recent contribution to the debate (Wong, 1973), published in a major economics journal; a contribution which, I argue, is more likely to heighten the Mystery than to solve it. Wong's paper is criticised in detail, for in two ways it seems to typify many preceding ones - showing a general leaning towards rather brusque dismissal of any rival position, after only cursory inspection of it; and embodying one of the common mis-statements of Friedman's case (for Friedman does not after all insist that predictive success is a sufficient criterion for choosing between theories). Now here it might seem that discovering the prevalence of misunderstandings of "The Methodology", though quashing my first, too rash, suspicions, does at least afford another easy, if different, answer to the Mystery. For even if Friedman is wrong, many may yet think him right when the main force of the opposition is directed against a position that is not his; whilst even if he is right, there may be many who suppose him wrong as long as his camp-followers, returning claims that far exceed their written warrant, still say they are speaking in his name.^{8, 9} With both attackers and defenders liable to mistake the issue in dispute, neither those who have accepted Friedman's challenge to previous

presumptions about economic method nor those who have instead rejected it may appreciate what considerations there are that might justly be brought against their view; and, neither side having taken to heart Mill's point that "he who knows only his own side of the case, knows little of that. His reasons may be good, and no one may have been able to refute them. But if he is equally unable to refute the reasons on the opposite side; if he does not so much as know what they are, he has no grounds for preferring either opinion"¹⁰, an absence of consensus is perhaps a natural result. There is a good deal in this solution, I think, and yet still it seems a somewhat superficial one: there must be more to the Mystery than that¹¹. For why should neglect of significant parts of "The Methodology" have become what amounts to the norm, with even supporters of Friedman often failing to recognise and criticise it? Chapter 4 takes up this question: it traces only the general implications of such neglect (with more detailed documentation of it relegated to Appendix A) and then starts to explore the possibility that Friedman tells his story only with studied ease, its seeming simplicity being practised too. It begins to appear that two very different themes may have been confused, not just in the reading, but in the very writing of "The Methodology". This seems to be a promising track to follow: pursuing it further, Chapter 5 at last offers what I believe to be the solution to the Mystery; whilst Chapter 6 moves on to relate the issues that have emerged as the tangle is unravelled to the questions which form the subject of Chapters 7, 8 and 9.

Appendix IIFootnotes to Chapter 2

1. By my reckoning, there are more than thirty articles in the main-stream of this methodological controversy. In the most recent flow are contributions by Coddington and by Rosenberg in 1972, and by Wong in 1973. (For the relevant papers in the A.E.A. Symposium see Samuelson (1963) and Nagel (1963)).

Appendix A gives a survey of the misunderstandings of "The Methodology" that have characterised much of this literature.

2. Friedman's 1953 essay, called "[The] Methodology" for short.
3. It might be questioned how far the methodological account these authors then proceed to give really does coincide with what Friedman's views are commonly taken to be, since they claim belief in "justifying [their] central assumptions in terms of their intrinsic realism". But they make an emphatic proviso, saying that "a close relationship should be maintained between reality and the assumptions of a theory, always provided that predictive success is not thereby diminished" (my emphasis). They do hold that "emphasis upon predictive success rather than upon description or explanation is the distinguishing characteristic of a positive science"; and that "the predictive test is ... the final arbiter" (pp. 173 and 174).
4. Kaldor goes on to argue for the adoption of an assumption of increasing, instead of constant, returns to scale.

5. On the moot point of how far the assumptions made in theories of the Cambridge school themselves escape Cambridge school criticisms of the assumptions of others, see Chapter 9.
6. In fairness, it has to be added that Leontief does distantly acknowledge, though only at a much later point in his Address, "the prevalence of ... the admittedly convenient methodological position according to which a theorist does not need to verify directly the factual assumptions on which he chooses to base his deductive arguments, providing his empirical conclusions seem to be correct" (p. 5). There too, though not discussing the point, he does make laconic mention of a factor that he believes makes such a position "untenable"; but the factor seems to be one that Friedman might accommodate too. See Appendix A.
7. In its "popular" form, Friedman's argument comes as counter-intuitive to those economists who have been trained to take the need for true assumptions as a commonplace. In being brought up in economics with the view that once one knew a theory's assumptions to be false one could dismiss the theory, bag and baggage, no doubt I was not alone. But one of the P. elements of the P.P.E. school should have warned me to be more wary here.
8. Penrose's description (1958, pp. 9-10) of dispute about the firm (dispute into which Friedman's essay enters), between those living in, respectively, "the high and dry plateaus of 'pure theory' and the tangled forests of 'empiric-realistic' research", comes

to mind here. She writes of "border skirmishes between the natives of the two areas ... supplemented by formal jousts in the medieval manner between noble knights of the opposing allegiances, each warmly defending his faith. These encounters have one remarkable characteristic - it seems strangely difficult for any participant to discover precisely where his antagonist stands, with the result that an uncommon number of thrusts seem to be made in one direction but countered from an entirely different direction, broad swords and rapiers forcefully cutting the air, without really clashing."

9. The attacks may have had force against some of these claims of camp-followers, of course; but still they need not carry conviction for readers of "The Methodology" itself.
10. Mill, On Liberty, Chapter 2 ("Of the Liberty of Thought and Discussion"). Mill continues by saying that it is not "enough that he should hear the arguments of adversaries from his own teachers ... That is not the way to do justice to the arguments, or bring them into real contact with his own mind. He must be able to hear them from persons who actually believe them; who defend them in earnest, and do their very utmost for them. He must know them in their most plausible and persuasive form; he must feel the whole force of the difficulty which the true view of the subject has to encounter and dispose of; else he will never really possess himself of the portion of truth which meets and removes that difficulty".

11. However, I here share Mill's, perhaps now unfashionable, belief that there can be progress through serious discussion (see Chapter 1). Some might argue that this is too sanguine: that conflict over "The Methodology" is likely to prove irresolvable, perhaps because it is, at bottom, conflict between Kuhnian paradigms (on which, see Chapter 3, note 30) or because the differences are essentially political ones (see the end of Chapter 4, and Chapter 6) or because contrary doctrines are held here as matters of faith (see Chapter 9).

Chapter 3MR. WONG GOES TO THE ZOO

In his recent paper, Wong takes a rather different view of the current state of the Friedman Affair. He makes the present Mystery seem a myth, holding (p. 312) that "many, perhaps most, economists consider the debate [between Friedman and Samuelson, a challenger of 1963] to be over, with Samuelson's position upheld"¹. Even if this consensus did indeed exist, however, Wong himself was threatening it; for his avowed purpose was "to show that no substantive issues were at stake in the Friedman-Samuelson dispute".² Given the alleged support for Samuelson's view of the case, against Friedman's, this would be tantamount to throwing down yet another gauntlet and reopening the dispute.³

On the other hand, were it really demonstrated conclusively that no substantive issues had been at stake after all, then at least a formal resolution of the dispute would have been provided; and in the longer term it ought to be possible - after a decent interval, during which disputants came to terms with having deluded themselves all along and having made such a dickens of a fuss over next to nothing - to close the file on the Mystery at last.

But does Wong succeed in demonstrating that no substantive issues were at stake? He might do so by showing that, on every critical issue held to be in dispute, agreement (albeit unrecognised) exists between Friedman's and Samuelson's views; or else by show-

ing that, wherever there is disagreement, the issues are not (however much they have been thought to be) substantive ones. But neither tack quite coincides with the line Wong takes: he makes just a fleeting gesture towards the first, elsewhere stressing the contrast he sees between the two supposed positions; but yet he does not argue consistently, in accordance with the second, that the issues truly in dispute are insignificant ones. The force of his argument is rather that there was never much point in trying to choose between what are indeed rival positions, since so much can be said against both.⁴ Now even if some reading of the initial claim does match this argument, still Wong's support for it, as I hope to show, is too flimsy: if this is so, then of course far from clearing up the Methodology Mystery, Wong's contribution is likely to intensify it.

In the paper, Wong, without blazoning the fact, takes us on a visit to a zoo: to what Hutchison/⁽¹⁹⁵⁹⁾calls "Professor Popper's private zoo of intellectual monsters". There he shows us six of the monstrous inmates, singly or in various family groups: we meet with essentialism, with conventionalism, and with descriptivism as an offspring of sensationalism,⁵ and we learn that apriorism is trying to insinuate a paw through the bars between him and instrumentalism. But now to the main event of our trip: seemingly the monsters are not, after all, to be found just in the captivity to which Professor Popper holds the key; for here is Wong, making copies of labels from two of the cages and hurrying off to pin them on two people in the outside world - people who turn out to be none other than Friedman (invested with

"instrumentalist") and Samuelson (dubbed "descriptivist").

Labels, of course, do often serve a purpose; but I doubt the usefulness of these labels here. For Wong's attribution leads him to interpret the claims of his authors in a very narrow and rigid way. Take the discussion of Samuelson as a descriptivist (aimed at dismissing his position - though in favour not of Friedman's but of Wong's). Here confusion is engendered by Wong's refusal to allow Samuelson access to the favoured sense he gives to words himself. Wong introduces (p. 321), with approval that is fairly customary, the argument that "all observational terms are theory-laden"; but then goes on to say that "the [descriptivist] view that knowledge consists essentially of observational reports is incompatible with the view that all observational terms are theory-laden". Yet, *prima facie*, there is no contradiction here. Clearly when someone labelled as descriptivist uses the term "observations", for Wong he can only be speaking of (mythical) non-theory-laden ones, observations supposed pure and simple; even though whenever Wong himself speaks of observations he means theory-laden ones, the only observations there really are on the view he subscribes to. Wong's argument that no non-theory-laden observations exist conflicts with descriptivism only where descriptivists claim, not just that knowledge consists essentially of observational reports, but also that observational reports consist of totally non-theoretical statements.⁶ And whilst extreme descriptivists may indeed make this latter claim, it cannot be shown that someone is an extreme descriptivist just by citing the former one.

A similarly unsympathetic reception is given to Samuelson's claim (1965, p. 1165) that what is commonly called an explanation is actually "a better kind of description and not something that goes ultimately beyond description". It is conceivable that, as Wong supposes (p. 319), Samuelson does "regard a theory to be only a description and not an explanation", taking the two categories as exclusive: one reading of Samuelson's claim could be that explanations as such are too airy-fairy to exist. Surely, however, one might rather take Samuelson to be saying that, though not every description is given the title of "explanation", theories, in explaining, are necessarily describing too: that because of this explanations can be seen as boiling down to a refined type of description; whence, if descriptions are looked on as real enough, explanations turn out to be thoroughly real as well. That he intends his claim to be read in this second way ⁷ is confirmed by context. For it is made in specific response to Garb's (1965) criticism (understandable following Samuelson's 1964 contribution to the debate, but still simply being echoed by Wong long after Samuelson's reply to it) that "Samuelson claims that scientists merely describe, they never explain ... [he] believes ... that since scientists do not possess ultimate explanations, they are therefore left with descriptions. But an explanation does not have to be 'ultimate'" (Garb, p. 1152). Samuelson retorts, "when Garb says 'an explanation does not have to be ultimate'", I think he is resaying what I said, not controverting it. An explanation, as used legitimately in science, is a better kind of description ..." etc.. Later in the same paper (1965, p. 1171) he

adds "what is called an explanation in science can always be regarded as a description at a different level - usually a superior description".

At least by 1965, then, Samuelson saw his remark that explanations are really just descriptions as simply another way of saying "it's no use searching for ultimate explanations", a point of view shared by Wong.⁸

Could it then be that any difference with Wong lies only in preferences about words, in that where Wong would say a theory is explanatory, Samuelson would prefer to say that it gives a superior kind of description? If so, then Samuelson's usage should at any rate be intelligible to Wong, who must himself allow the term "descriptive" to include an explanatory element: all descriptions containing observational terms must be theory-laden, according to Wong; and, as he insists that theories must be explanatory, descriptions will be, so to speak, explanation-laden too. However, Wong claims to have established independently that Samuelson, protest as he may to the contrary, is actually precluded from holding an explanatory view. According to Wong, the theories Samuelson sanctions fail to fulfil generally accepted requirements for explanatoriness. In particular, a requirement that "the explanans [defined as 'the set of statements which forms the explanation'] must have testable consequences in addition to the explanandum [defined as 'the set of statements which describe what is to be explained']" (Wong, p. 317) is held to be violated by the theories Samuelson approves. Wong has earlier presented Samuelson's part in the Friedman Affair as developing from the proposition that the

minimal assumption set of a theory is logically equivalent to its complete consequence set. Now, Wong's claim is that, as theories on this view are statements of logical equivalency, "the explanans (the axiom set) is just a restatement of the explanandum (the consequence set)" (p. 317) and the theories lack testable consequences beyond the explanandum. But this argument is at best incomplete: it hinges on Wong first switching to identifying the explanandum just as "the consequence set" (instead of as the set of statements describing what is to be explained); and then proceeding to interpret this in Samuelson's case as the complete consequence set, for which the logical equivalency claims are made. Yet by contrast, the "consequence set" with which the explanandum in a theory fulfilling Wong's requirement for explanatoryness is to be identified is only a subset of the theory's full logical consequences. Before assenting, then, to Wong's criticism of Samuelson - that all he has to offer is restatement, never explanation⁹ - we would certainly need to know that for Samuelson the explanandum of a theory, stating what, in his terms, is to be described in a superior way, must indeed be that theory's complete consequence set.¹⁰ Since Wong fails to assure us of this, it seems to remain possible that Samuelson would in practice admit those theories that Wong calls explanatory:¹¹ Wong's quarrel with him here is merely verbal if what distinguishes a better from a lesser kind of description for Samuelson is that which distinguishes an explanation from a "mere" description for Wong.^{12, 13} Paradoxically, oversimple labels may complicate the issue.

In Friedman's case, Wong (p. 314) finds it "quite evident" that his instrumentalist label applies. But does it really fit? That Friedman is solely concerned with the immediate success of predictions has been a popular view; and it is a view of this sort that Wong endorses when he labels Friedman instrumentalist, seeing him as advancing principally the stark claim that a theory, being "merely an instrument for prediction . . . is tested only by the conformity of its predictions with observable reality" (Wong, p. 314). Though the view has been popular, however, it has also been, I think, a fallacy that has contributed to the persistence of the Mystery: lack of agreement on the value of "The Methodology" can to some extent be explained by the fact that this prevalent interpretation leaves totally out of account the qualifying statements that Friedman makes.¹⁴ True, he announces fairly near the outset (p. 8) that "the only relevant test of the validity of a hypothesis is comparison of its predictions with experience", which may sound categorical.¹⁵ But he is careful to italicise "validity" here, surely because this first remark is to be taken in conjunction with a second, namely (p. 9) that "the validity of a hypothesis in this sense is not by itself a sufficient criterion for choosing among alternative hypotheses", filled out by the later (p. 10) "choice among alternative hypotheses equally consistent with the available evidence must to some extent be arbitrary, though there is general agreement that relevant considerations are suggested by the criteria 'simplicity' and 'fruitfulness', themselves notions that defy completely objective specification". Wong's neglect of these qualifying remarks is particularly flagrant; for after asserting that Friedman

is an instrumentalist, he goes on to say (p. 315) that "if Friedman's position is interpreted as instrumentalist, it can easily be seen that testability (in Friedman's sense) is both a necessary and a sufficient condition for the acceptability of a theory."¹⁶ Remembering Friedman's emphasis on the insufficiency of the predictive test, might one not reverse Wong's reasoning here and conclude that, if being an instrumentalist is what Wong says it is, then Friedman is not an instrumentalist after all?¹⁷

Nor can the above remarks of Friedman easily be treated as an isolated anomaly. For instance, he later suggests (p. 17) comparing the promising but imperfect predictive performance of a theory with the accuracy of an alternative one "which is equally acceptable on all other grounds": that is, equally acceptable on grounds other than predictive success, such as, presumably, "simplicity" and "fruitfulness".¹⁸ Or again, it is an, admittedly pragmatic¹⁹, part of his position in "The Methodology" that (p. 17) if "there exists a theory that is known to yield better predictions but only at a greater cost ... the gains from greater accuracy, which depend on the purpose in mind, must then be balanced against the costs of achieving it"; for example (p. 18), "in the particular case of falling bodies ... it does not always pay to use the more [accurate but complex] general theory because the extra accuracy it yields may not justify the extra cost of using it". Yet, according to Wong, Friedman's concern for successful prediction is "overriding" (p. 315) and his pursuit of it "single-minded" (p. 324). Then Friedman is said to hold the crude view that "a theory must be

the most successful predictor before it can be given serious consideration": Wong accepts (p. 315) the claims that Friedman must deny "a yet unfalsifiable theory from careful consideration" and that his methodology "rules out such careful consideration of fruitful alternative theories"²⁰, where again Friedman's position includes subtleties that Wong and others appear unaware of. For Friedman allows (p. 29) that a hypothesis "gains indirect plausibility from the success for other classes of phenomena of hypotheses that can ... be said to make [the same] assumption; [because] at least, what is being done here is not completely unprecedented or unsuccessful in all other uses". He illustrates this with an example, remarking (p. 30) that "of course" indirect evidence of this sort is not conclusive; but that, although "the decisive test is whether the hypothesis works ... a judgment may be required before any satisfactory test of this kind has been made, and, perhaps, when it cannot be made in the near future, in which case the judgment will have to be based on the inadequate evidence available" - not a rapturous admission of the claims of "a yet unfalsifiable theory", but their admission nonetheless.

However, Friedman's interest in prediction is already labelled for Wong as instrumentalist and obsessional, as automatically subject to the criticism (p. 324) that "the instrumentalist's particular obsession with predictions disregards the explanatory content of a theory".²¹ Now it is true that some of Friedman's claims for prediction may sound extravagant, taken in themselves: for instance, he does say (p. 7) that arriving at "valid" (though also "meaningful") predictions

is "the ultimate goal of a positive science"²². But theories whose predictive power is securely founded might also explain well, and vice versa; and it is possible that Friedman, far from disregarding explanatory content, rather reasons that prediction and explanation are symmetrical and intends his claims to apply to both. Several statements offer support for this interpretation (for example (p13), "for [the predictive] test to be relevant, the deduced facts must be about the class of phenomena the hypothesis is designed to explain").²³ Of course, Wong might retort that Friedman's error comes in failing to recognise the distinct features of explanation: that virtually identifying explanation with prediction is just as bad as ignoring it altogether. But is it self-evident that this is a heinous crime? It has been at least philosophically respectable to treat explanation and prediction as two, temporally distinct, sides of a single coin since, in 1948, Hempel and Oppenheim argued for symmetry between them.^{24, 25.}

Or again, Wong (p. 315) seems to take Friedman's merely comparative standard of predictive failure (a theory failing the test "if its predictions are contradicted ('frequently' or more often than predictions from an alternative hypothesis)" (Friedman, p. 9, Wong's italics)) as evidence that Friedman's interest always stops short, with the instrumentalists, at determining "the limits of [a theory's] applicability", and never extends to concern with enlarging "our understanding". But, as before, Friedman may be in better philosophical company here than Wong has recognised²⁶; for Popper, a persistent critic of "the obscurantism of instrumentalism" (Popper, 1963, p. 100),

yet draws on an idea of approximation to truth, or "verisimilitude", whose "comparative use ... is its main point" (Popper, p. 234). Popper writes (p. 235) that "ultimately, the idea of verisimilitude is most important in cases where we know that we have to work with theories which are at best approximations - that is to say, theories of which we actually know that they cannot be true", adding "this is often the case in the social sciences". And he judges that "in these cases we can still speak of better or worse approximations to the truth (and we therefore do not need to interpret these cases in an instrumentalist sense)". But then, no more need Friedman.

It is not, then, so "evident" as Wong supposes that Friedman is a die-hard instrumentalist: his views seem both more subtle and more respectable than Wong's stereotyping label suggests. Nor, were Wong now to acknowledge Friedman's moderate statements, could he readily support the label by changing his plea to one that whether or not to adopt this tag is simply a matter of taste, of choosing between alternative interpretations either of which might be valid and which, as they stand, appear equally plausible. For if Friedman is seen as Wong's out-and-out instrumentalist, seeking to present predictive success as the single, clear yardstick for judging theories, whatever can explain his various deviations from the task? Taking this extreme and unqualified position to be Friedman's, with "The Methodology" written expressly to express it, involves construing his then errant statements there not (unproblematically)²⁷ as genuine reservations, voiced on purpose, about the rigid view, but rather as unwitting contradictions of

it; and what is there then to account for Friedman repeatedly lapsing into them? Could all the passages out of accord with the stereotype Wong would impose be merely a product of slovenly thought, the casual outcome of carelessness? - surely this suggestion lacks appeal.²⁸

And until Wong can point to some more positive cause for these divergent statements, his interpretation must lack appeal too.²⁹

Thus, Wong's zeal for labelling has led him to caricature both sides in the "Friedman-Samuelson dispute". In consequence, he presents the dispute as having almost inevitably been a barren confrontation between rival philosophies, with a breakdown in true communication between the advocates of each.³⁰ For instance, the "possible Friedman defense against Samuelson's critique" that Wong advances (pp. 315, 316) (and he advances only one) is a peremptory declaration of rejection, rather than a reasoned reply: in Wong's view, "Friedman would certainly reject the Theorem [of Samuelson, that 'it is a contradiction to maintain that all consequences can be valid and the theory and the assumptions not valid' (Wong, p. 315)], since he does not accept Samuelson's view of the desired relationship between a theory and its assumption and consequences as one of logical equivalence"; which, without the addition of any considerations which Friedman might have put forward to show that the relationship can be different, comes to little more than saying that Friedman would reject the Theorem because he does not accept it. Moreover, given Wong's wording of this Theorem, it seems by no means "certain" that Friedman would in fact reject it: mightn't he first jib at the phrase

"all consequences", as lacking clear meaning as it stands³¹; but then be willing to accept the Theorem, as true but of no practical significance, if the meaning of "all consequences" is specified as "the complete logical 'consequence' set"?³² Wong also holds that "similarly Friedman would reject the Corollary [that 'it is absurd to maintain, in the case where only some of the consequences are valid, that the theory and the assumptions are important though invalid. The parts of the theory set and the assumption set corresponding to the invalid part of the consequence set should be eliminated' (Wong, p. 316)] since for his instrumentalism it is irrelevant that the theory or its assumption set is invalid since what matters is whether the theory gives sufficiently accurate predictions for the purpose at hand." Now in this context I should have said that there is a sense in which it surely is relevant for Friedman that the assumption set is invalid, it being for just such, often scorned, sets that he is anxious to claim a role. Probably, however, Wong's meaning is that Friedman would reject the Corollary because he does not share Samuelson's professed concern with arriving at valid assumption sets. But then the suggested defence is a mere reiteration of the position that the Corollary has claimed to be "absurd".³³ Dismissing Samuelson's objections out of hand is apparently for Wong virtually all an instrumentalist needs and is able to do³⁴, to show "that there is not much substance to Samuelson's critique from an instrumentalist point of view" (p. 318)³⁵; and this leaves the coast conveniently clear for Wong to put forward both his own objection to instrumentalism and also his "independent ... critique of Samuelson's critique" (p. 315). In a rather similar way,

though Wong accepts Klappholz and Agassi's account of what Friedman's position is, he yet seems to present their criticisms of this stated position as failures to sympathise with, rather than objections to, it³⁶; so that, manipulating his puppet-like Friedman once more into reply by simple reassertion of various claims, he again restricts "freedom of entry" into any significant discussion.

Despite his insistence of holding a monopoly of worthwhile independent argument, however, Wong's attempt to show that both sides in the dispute have been misguided is vitiated, not only by his misfitting labels, but also by a curious failure of nerve. For though he at least aims a direct attack at descriptivism, when it comes to instrumentalism he throws up the sponge. In defeatist vein, he writes (p. 323) that "descriptivism was shown to be untenable in view of logical and epistemological difficulties but instrumentalism cannot be defeated in a similar fashion. In fact no refutation appears possible". This is hardly the soundest basis from which to campaign for the adoption of a preferred third view. It is also hard to reconcile with Wong's earlier presentation of his case: so lenient a treatment of instrumentalism seems partisan, when one tenet of instrumentalism conflicts with the set of requirements for an explanatory theory that Wong brought to bear against Samuelson. For one of these requirements (p. 317) was that "the explanans must not be known to be false"; whilst Wong's instrumentalists hold, in contrast, that "it is superfluous and irrelevant to test assumptions as they are merely tools or instruments which are judged by their ease or convenience in use" (p. 314) and

that "allegedly false statements in the theory are not problematic if the theory can give sufficiently accurate predictions" (p. 315). Thus, having assumptions known to be false is sufficient in itself to damn a theory for Wong, but is insufficient to do so for his instrumentalists; and, where Wong merely bemoans an instrumentalist "disregard" for existing explanatory content³⁷, we might expect him instead to reject their position categorically, just as he does the descriptivists¹, on the grounds that theories it warrants can actually violate his required "explanatory" standards.

But perhaps Wong's uncharacteristic reluctance³⁸ to claim here a clinching of the case against a rival doctrine stems from realisation that he could face two problems in doing so.³⁹ In the first place, in his anxiety to dismiss Samuelson's challenge to Friedman, Wong has taken a surprising line on the role accorded to false assumptions in "The Methodology". Unlike some critics of the essay, he seems aware of the ambiguity in Friedman's notorious passage, commending hypotheses that are "descriptively false in [their] assumptions" (Friedman, p. 14)⁴⁰, between the views that, on the one hand, a theory's assumptions ought to state things which are not true of the assumptions' subject-matter and that, on the other, they ought not to state everything which is true of it; and his interpretation favours the second view: he writes (p. 316) that Friedman's intention is to praise a theory not, as Samuelson suggested, for "its shortcomings, i.e. its unrealism" but instead for the virtue of "simplicity produced by an abstraction". Such a reading, on the face of it, sits rather oddly with Wong's earlier

claim that Friedman's pursuit of predictive success is "single-minded". All the same, he is surely right here not to follow those critics who unquestioningly attribute to Friedman the former of the two views, neglecting the latter; for to do this would, I think, be to misrepresent further Friedman's case (in a way to be taken up in Chapter 4). Wong is right, that is, to see Friedman's argument^{here}/as being first and foremost for omission, for telling less than the whole truth.⁴¹ Telling less than the whole truth does not, however, necessarily carry with it adherence to telling nothing but the truth, a point that has been overlooked in parts of the literature: this has been yet another source of misunderstanding. Not stating everything which is true of a theory's subject-matter surely does not exclude (though it may not enjoin) stating something untrue of it too.⁴² But if this is so, then, in view both of Friedman's denial that independent testing of a theory by tests of its assumptions can be adequate, and of the fact that every one of his examples has an untrue assumption which would indeed be outlawed by Wong's entirely strict requirement on truth, (apart of course from the fact that, if Friedman were one of the instrumentalists who fits Wong's descriptions, he must presumably in any case share their willingness to tolerate false assumptions), why does Wong fail to invoke his truth requirement here to clinch a case that is hostile to Friedman? It seems to me likely that he has rejected one frequent misinterpretation here only to move to the other, that tends to err in the opposite direction. I suspect that, his prohibition on falsehood being violated by sins of commission rather than ones purely of omission, he has fancied that it will necessarily be entertained in

company with any argument that omission there should be; so that, on his interpretation, Friedman's argument would in fact rule out any scope for false assumptions.⁴³ It may be partly because of such a reading of this passage of an alleged instrumentalist, then - though still strangely in view of the evidence elsewhere - that Wong hesitates to reject instrumentalism on the grounds of infringement of his stipulation about truth.

In the second place, there is the problem for Wong that the laws of logic, as Wong understands them, seem, taken in themselves, to support his instrumentalists' attitude rather than his own doctrine that "the explanans must not be known to be false". To some extent, Wong may appreciate this: in arguing against his descriptivists' ideal of logical equivalence between assumptions and consequences, he does say (p. 317) that (presumably, for the standardly "incomplete" assumption and consequence sets) "the possibility remains that the theory, its assumptions, and its consequences can have different degrees of realism or empirical validity". But the amplification that follows stops curiously short: he cites only the Modus Tollens rule, to show that "of course it is wrong to say that the consequence set can be false and the axiom set, true". This leaves it to the instrumentalists to invoke Modus Ponens, making a seemingly parallel use of it to show that "of course it is right to say that the axiom set can be false and the consequence set true" - an apparent vindication of their claim and rebuttal of Wong's requirement. Perhaps Wong might then retort that to say this is "right" so far as it goes but does not, in the context of theory

choice, go anything like far enough: that patterns of argument which are valid from a narrow standpoint of formal logic will not necessarily be acceptable from that of living language (even if invalid patterns are unacceptable); and that, for a theory to succeed in explaining, unimpeachable logical validity, though necessary, is by no means sufficient.⁴⁴

But if his rejection of the known false explanans (a rejection he presents as an integral part of the "explanatory" view) is to be sustained, then some such additional argument must be offered - an addition that Wong gives no hint of having at hand, and that could of course, assuming parity of treatment with his descriptivists,⁴⁵ be used to defeat instrumentalism after all. His acknowledged inability to floor the instrumentalists appears, then, as something of an embarrassment: small wonder perhaps if, seeking a way out of this tight corner, he resorts to the desperate stratagem of abuse.

Reckoning that refutation of instrumentalism, and so of what he looks on as "Friedman's instrumentalism",⁴⁶ is impossible but wanting nonetheless to discredit it, Wong styles it "fundamentally anti-intellectual" (p. 323). But does this go much beyond telling us that Wong disapproves of it himself, that he believes it to be somehow distasteful? Exactly what are we to understand by "anti-intellectual" here; and what is to persuade us that an "intellectual" position, in these terms, is always to be preferred?⁴⁷ Perhaps one possibility is that Wong intends his "anti-intellectual" charge as an oblique reference to Popper's indictment of instrumentalism as, though attractive, yet "obscurantist" (Popper, 1963, pp. 100 and 113); but

if he does, why no direct reference, and what has stopped him from presenting the point, as Popper appears to do, as backed by arguments that are sufficient to defeat an instrumentalist position? (Could this be because Wong has sensed that Popper's argument may not after all have much force against what Friedman is saying?)⁴⁸ Wong giving no clear guidance on what "anti-intellectual" is meant to convey, and no adequate support for his charge that the word applies,⁴⁹ his rejection of instrumentalism hardly seems likely to persuade any hitherto unconverted.

Since Friedman himself presents very ingenious arguments, there is something especially unsatisfactory in just branding them "instrumentalist" and then brushing them aside as "anti-intellectual" in favour of a, supposedly contrary, set of assertions. So cavalier an approach cannot provide solid support for Wong's claim that "no substantive issues were at stake in the Friedman-Samuelson dispute"; and it seems more likely to perpetuate the Methodology Mystery than to yield a solution. As Wong confesses he cannot refute even a hard-line instrumentalism, let alone Friedman's more guarded views, his method is not well calculated to deter extremists from adopting any false assumption they may please; whilst Samuelson's supporters, thinking they have only to dodge Wong's "descriptivist" tag to regain their status, might persevere, less prudently than their mentor,⁵⁰ in jettisoning every theory with any false assumption whatsoever and rejecting Friedman's message lock, stock and barrel. It is in resting on his labels that Wong goes wrong.

The question whether Friedman is right remains. Does he make out his case? And why the mysterious difficulty in securing consensus about this in the past? - Just what is the case on "telling the truth" that has been so widely misunderstood? The next two chapters tackle these issues.

Appendix IIIFootnotes to Chapter 3

1. His assessment runs counter to those of Agassi (1971) and of Rosenberg (and to Leontief's: see Chapter 2, note 6).

Wong also holds (p. 312) that the debate has been "vague and confusing" and so "of little apparent significance to the day-to-day work of the practicing economist"; whereas my impression is that the undeniable confusion may have fostered some uncritical implementation of the views associated with each side.

2. Here (and below), I take Wong to mean that no issue is "substantive" in the popular English sense of "weighty" or "significant" (the third meaning Webster's give: "enduring; solid; firm").

(Although Wong is to argue later, inter alia, that the positions of Friedman and Samuelson in this dispute derive from wider methodological positions, I am sure that he does not mean "substantive" in the (fairly standard English) sense of "independent" - a sense listed as obsolete in Webster's - since his claim wouldn't then fit into the context in which it occurs).

3. Moreover, Wong goes on to argue (p. 318) that "Samuelson's presentation of the relationship between a theory and its assumptions and consequences is wrong".
4. It is in a footnote (p. 318, n.17) that Wong states that "the views of Samuelson and Friedman are not mutually exclusive" (he next

cites Boland's view that the debate between the two is a "family dispute" between two "conventionalists"). But in general he stresses contrast ("Friedman ... does not accept Samuelson's view of the desired relationship between a theory and its assumptions and consequences" (p. 316); "instrumentalism ... goes 'beyond the facts' ... descriptivism ... designs a theory not to go beyond the facts" (p. 324)) - the only feature that he brings out as common to both views being their inferiority to his own preferred one ("the choice, then, is not between instrumentalism and descriptivism but between them both and the view that a theory is explanatory and informative" (p. 324); similarly (p. 312), "the methodological choice is not one between the Friedman view and the Samuelson view ..." etc.). Without wanting to label either Friedman or Samuelson as "conventionalist", I think the prospects for showing that there is a considerable measure of agreement between them would have been better (see page 3.12 and note 50; and also note 26); and I believe that some of Boland's reasons (not given by Wong) for placing them in the same camp are important ones (see Chapters 6 and 7).

Even if Wong were right in his apparent belief that the dispute has not concerned the most important methodological issues, still it would not follow (as he seems to suppose) that the issues that are involved are unimportant (see notes 13 and 37).

5. Strictly, Wong's descriptivism is not an inmate of the Popper zoo proper; though it may be that it is merely reclassified there - as

observationalism (Popper, 1963, p. 123). Wong seeks to distinguish descriptivism (the view "that [theories] are only descriptive") from what is meant by the, Wong says, "usual" term, "descriptive" (a term Popper often uses) - that is, from "the view ... that theories are descriptive" (Wong, p. 319, n. 19); but I suspect this has led him into error. (See pages 3.3 to 3.6; also notes 8 and 30).

Nagel (1961) does mention the term "descriptivism" but avoids making much use of it - noting, as Wong fails to do, that "describe" may have a wide range of meanings (see note 8). Moreover, the position with which he associates the term is less rigidly conceived, occupants of it, unlike Wong's descriptivists, differing from instrumentalists only (p. 152) "over preferred modes of speech" (see note 30).

6. Popper, whom Wong relies heavily on, qualifies a claim that "all terms are theoretical to some degree" with the addition "though some are more theoretical than others" (1963, p. 119) - leaving room for Samuelson to argue for theories being grounded in statements involving the 'less' theoretical ones.
7. Wong himself (p. 319) also presents Samuelson as claiming that "explanations turn out to be just better descriptions" (see also Wong, p. 317). But he cites this as one of the reasons a descriptivist has for "rejecting the view that theories are explanatory" - even though his descriptivists hold that theories are descriptive. (Of course, they also hold that theories are "only descriptive";

but what need this exclude?) See below.

8. And by Popper. But, unlike Wong, Popper recognises that rejections only of ultimate explanation as an aim are sometimes expressed, confusingly, in the form of very strong claims for description. However, it is instrumentalists that Popper is writing of when he suggests that rejection of ultimate explanation by essences may be what is intended when the formula "aim at description rather than explanation" is used. (He writes (1963, p. 104, n. 14) that the instrumentalists' meaning is that "theories which do not describe in this sense [i.e. do not describe "the ordinary empirical world"] do not explain either, but are nothing but convenient instruments to help us in the description of ordinary phenomena"). Nagel (1961) also draws attention to the misunderstandings connected with strongly worded claims for description, pointing out that the narrowness of the claim that sciences never explain but merely describe depends critically on the meaning given to "describe" (is it, for instance, allowed that we can "describe" what may never take place?) See also notes 5 and 30.
9. Can no restatement ever count as some sort of explanation? Perhaps there may be confusion about what the restatement restates. Wong's opposition between restatement and explanation seems most likely to hold if it is sentences, rather than statements (in the terms of Cartwright (1962) and Lemmon (1966)), that are being restated, for here the scope for explaining in restating does seem relatively slight. Taking each "type" sentence to be a specific

sequence of words, then the sentence can only be stated (uttered) by means of that particular word-sequence (in a "token" sentence of that "type"); and restating the sentence must mean repeating it, word for word (in a second "token"). (Even here, though, I am not sure that the repetition must be mere repetition: if changes in intonation are taken into account (same words: different meaning?), mightn't even restatements of this kind offer some explanation?) But, by contrast, a statement can be stated (made) by means of tokens of different "type" sentences; and hence it could be restated in words that differed from those first used: may not the second way of stating it be explanatory? ("We must petition Mr. Jenkins"; "Mr. Jenkins?"; "We must petition the Home Secretary"). Similarly, in Quine's terms (Quine, 1970), the same "eternal" sentence can be expressed through different "non-eternal" sentences, if there are relevant differences in their context of utterance - e.g., ^{in the} exchange above, it being known on the one hand, and not known on the other, that Mr. Jenkins is the Home Secretary - and again this seems to give some scope for explanation. (Furthermore, what is the case about X could perhaps be restated - though this may not be "restatement" in Wong's sense here - by means even of different statements (different "eternal" sentences), "redescribing" X, which surely might often then be counted as explanations).

10. The criticisms made of Samuelson's views through Wong's schema of p. 319 seem to rely heavily on Wong's concentration on the

lines running horizontally, to the neglect of moves in (p. 319)

"the northerly direction ... [of] increasing generality or universality". Yet the schema might surely be held to embody a procedure Wong approves (even if he would not approve of the terms Samuelson might describe it in): in Wong's terms, may we not "explain" the consequences C of theory B, in the schema, by the more general theory B+ ? - a theory which would then of course have "testable consequences beyond the explanandum" (since C is only a subset of C+, the complete logical consequence set of theory B+).

11. Wong does not show (see the argument above and note 12 below) that Samuelson is precluded from employing criteria for "better" descriptive power that are akin to Wong's own (p. 317) for explanatoriness. And whilst some statements on method may have a clear enough meaning to be criticised in their own right, independently of how (or whether) their makers put them into practice, Samuelson's claim that an explanation is "a better kind of description" is surely not sufficiently specific to serve in itself as a firm target for Wong's attack.
12. A later argument of Wong against Samuelson (Wong, p. 320) might be mobilised here, to show violation of a second requirement for explanatoriness, were it not that this argument too appears incomplete. The claim would then be that if the descriptivist desire "to ground theories in observational statements" were realised, the requirement that the explanans of an explanatory

theory must include at least one universal law would be infringed, because "an unrestricted universal statement is not equivalent to a finite conjunction of observational statements". However, an answer would be needed to a natural retort of the descriptivist: that a universal statement may nonetheless be equivalent to an infinite conjunction of observational statements.

13. However, though I think this may just be a matter of emphasis, Samuelson probably does dwell unduly on what would hold for notional "logically complete" consequence sets (see note 32). I also agree with Wong that a Quinean point may have some force against Samuelson's objections to Friedman's case (see note 34). (Why, however, doesn't Wong count these issues in the dispute as "substantive" ones?)
14. The neglect of some of the statements Friedman makes which has characterised much of the debate is documented in greater detail in Appendix A.

Unless an argument strong enough to justify passing over these qualifications is provided first, setting them aside surely seems unfair; but yet very few who agree with Wong's interpretation here offer any argument in support of it, many not even appearing to recognise that the qualifications are there. Since the qualifications are made, it seems to me that in fairness some mention must be made of them (and that this would be so, even if there were compelling reasons for subsequently setting them aside). However in Chapters 4 and 5, where a deeper answer to the

Mystery is sought, I ask why there should have been so many Mystery-augmenting omissions of significant parts of Friedman's case - and allow that they may not have been altogether unprovoked.

15. See also page 3.9 below.
16. Since Friedman does not, I think, use the term "testability" in "The Methodology" (and Wong gives no reference here), it might seem hard to know just what Wong understands by "testability (in Friedman's sense)". But Wong surely must intend this as a reference to Friedman's (p. 8) statement about testing the validity of a hypothesis by comparing its predictions with experience (having taken note of Friedman's (p. 9) phrase "the validity of a hypothesis in this sense" without taking note of the rest of the sentence it occurs in).
17. Unless there is a statement to the contrary, the term "instrumentalism" is used below to mean "instrumentalism as defined by Wong": for the most part, I assume for the sake of argument that instrumentalism is what he says it is.

Perhaps instrumentalism need not be what Wong says it is; and in that case it is conceivable that Friedman could legitimately be described as instrumentalist. But then my objection would run parallel with that to Wong's treatment of Samuelson: Friedman's being an instrumentalist in some other sense would give no grounds for attributing to him, and criticising him for, views peculiar to the instrumentalism of Wong's particular definition. If Wong has defined instrumentalism adequately, then Friedman isn't an

instrumentalist; or again, if Friedman is an instrumentalist, then Wong hasn't adequately defined instrumentalism. Either way, Wong's criticisms of the position he defines needn't affect Friedman.

In fact, Popper also defines instrumentalism fairly narrowly (and more narrowly than Nagel does), when he writes (1963, p. 101) that "the instrumentalist view asserts that theories are nothing but instruments". But he recognises, as Wong appears not to have done here, that less narrow views will not fall under a narrow definition. Thus, he allows that the claim that theories do serve as instruments is consistent with non-instrumentalist positions, pointing out that disagreement between instrumentalists and those who held "the Galilean view ... that [theories] are not only instruments but also - and mainly - descriptions of the world" would not be settled by showing that "theories are instruments" since "both were agreed on this point" (p. 101). He also says (p. 63) that "that theories may be used in this way ... as practical instruments or tools for such purposes as the prediction of impending events ... cannot be doubted". I doubt if Friedman need be labelled "instrumentalist" on Popper's definition either. (See especially note 19; but also notes 18 and 48, and pages 3.9 and 3.10).

18. Whereas I take this reference to "other grounds" as additional evidence that Friedman is not concerned with immediate predictive performance to the total exclusion of the other considerations often canvassed here, some critics (typically citing out of context his

earlier (p. 8) remark about the validity of a hypothesis) have seized on it as instead betokening inconsistency. (Might Wong be doing so too? See below.) Though loud in denouncing exclusive concern with predictive success, and in proclaiming the importance of recognising the various other factors, they yet insist that Friedman's own reference to "other grounds" has to be ruled out of court. But they appear to take it as a datum that his concern with prediction is an exclusive one.

However, it might be argued that such "other" factors as Friedman does admit are still seen by him as contributing in a wider context to the ability to predict (to the ability to predict well in areas of increasing scope in the future, perhaps given constraints on time and resources to spend on predicting). The sketch he gives of how the criteria of simplicity and fruitfulness apply tends to bear out this suggestion; and, after all, he does write (p. 7) of the development of theories yielding "valid and meaningful" predictions as "the ultimate goal of a positive science". But since exclusive concern with developing prediction in the wider context would, on this view, include concern with simplicity and fruitfulness (and might allow that immediate predictive success is not the be-all and end-all), would it any longer be objectionable to Friedman's critics, as leaving important factors out of account? Perhaps the protest might now be made that a further consideration is crucial - namely, a theory's explanatory power. But Friedman apparently sees prediction as going hand-in-hand with explanation - a view that some philosophers support. See below, page 3.9.

Immediately after saying that Friedman's instrumentalism is "quite evident", Wong does mention "apparent ambiguities and inconsistencies in his essay" - now claiming that these "can best be sorted out" (p. 314) by treating Friedman's view as instrumentalist. Is there an implicit reference to Friedman's recognition of "other grounds" in this? (All that Wong adds is that "all [Friedman's] methodological prescriptions" are "subsidiary" to the "overriding methodological maxim ... of successful prediction").

19. Several commentators have drawn attention to Friedman's pragmatic talk here of whether it will "pay to use" a theory, rather than of whether there are grounds for believing the theory. (See also Chapter 4, note 41, on the ambiguity in Friedman's (p. 9) phrase, "the hypothesis ... is accepted ..."). But it is of course usual to take practical considerations into account in some contexts: Samuelson (in 1965), Nagel and Popper all also allow that, in Popper's words (1963, p. 113), "for instrumental purposes of practical application a theory may continue to be used even after its refutation, within the limits of its applicability".
20. Wong's expression of claims made about Friedman's position in Klappholz and Agassi's criticism of it (1959). Wong accepts the claims; but does not, it appears, accept them as criticisms, saying (p. 315) that "we can understand Friedman's position". See page 3.14, and note 48. See also Chapter 4, for some support of Klappholz and Agassi's main, and more favourable, thesis about "The Methodology". (Wong does mention

their contention "that Friedman's position is essentially a critical one" (Wong, p. 314), saying that their interpretation is "an alternative" to his instrumentalist one; but is it? - and, if it were, how could it be "evident" that Friedman is an instrumentalist?

See p. 3.11 above; note 18; and Chapter 4).

21. See note 37.
22. Does this contradict the admission of "other grounds" than predictive success for accepting a theory? On simplicity and fruitfulness as possible means to a predictive goal, see note 18.
23. Coddington believes that "the structural equivalence of explanation and prediction can only be maintained by a rather drastic distortion of the customary concept of explanation" (1972, p. 4); and he remarks that "it may be in half-conscious acknowledgement of this distortion that Friedman always puts the word "explain" in inverted commas. The point is that if the word "explain" is being used in its customary sense, why is it necessary to put it in inverted commas?" My example from page 13 of "The Methodology" puts the lie to Coddington's strict "always" (as would other examples: e.g. Friedman, pp. 12 and 28); but it would be quibbling to make much of this, since these counter-examples seem to come either soon after an occurrence of "explain" that is in inverted commas, or else in phrases in which "explain" was marked in this way in a previous use. But I think there are several possible reasons for this "encapsulation", apart from that Coddington suggests. (Consider, for instance, Friedman's use of inverted

commas with the term "assumptions", where he holds (p. 23) that "the very concept of the "assumptions" of a theory is surrounded with ambiguity" (admittedly, however, his own use of this term too has been the subject of criticism; see Appendix A); or his use of them with "realistic"). Although well aware that what we count as explanatory, and what we can justify so counting, have been matters of dispute, Coddington evidently, but I think rashly, assumes here that there is just one clear sense of "explain" - the "customary" one - and that there is nothing loose or potentially misleading about this customary usage. (Later in his paper he is, however, more hesitant.) See below, and note 25.

24. If not before, Coddington notes that Marshall subscribed to the symmetry view - though, according to Coddington, he did so on grounds that Friedman could not admit.
25. It might be objected that none of this respectability can attach to Friedman's position, because in "The Methodology" he fails to draw the fairly standard distinction between secure prediction in accordance with lawlike, potentially truly explanatory, regularities (the prediction for which any symmetry claims could best be made) and prediction based merely on statistical ones. But the mere lack of explicit reference to this distinction is not in itself decisive evidence of failure to accept it. There is perhaps indeed something rather bald in the particular observed or supposed regularities that Friedman offers as theories, of market behaviour in "The Methodology", of national income determination

in his Quantity Theory writings; but, on the other hand, Friedman would doubtless have plead that, if this is so, it is just a symptom of economics still being at a primitive stage, compared with other "positive" sciences. Though not claiming that Friedman's position altogether coincides with the Hempel-Oppenheim one, I would argue that his implicit suggestion of treating explanation and prediction together is not, given Hempel-Oppenheim, patently indefensible. See notes 18 and 48.

I am not arguing, then, that Friedman's approach to explanation is correct: to hold views that are respectable is not necessarily to hold views that are right (see also note 26). But many commentators have surely, like Wong, at the least been too swift in announcing dismissal. Even Coddington, who advances beyond many critics here in his recognition of the Hempel-Oppenheim view, apparently assumes that Friedman's position must be a shallow one. True, Coddington does (pp. 4, 5) invoke the well-worn example of the daily rising sun in order to illustrate the objection that, whereas an event can be predicted on the basis of a generalisation that "may be seen merely as a de facto regularity", it can hardly be "explained by subsuming it under a de facto regularity", explanation requiring in addition "some sort of causal narrative" - an objection that does not seem to me to be wholly convincing (do we in fact ever predict that the sun will rise merely on the basis of a de facto regularity? Can we predict confidently where only de facto regularities do seem to be involved (tossing a coin: three heads in a row ...)? Couldn't it be said that, in so

far as primitive man (or Coddington's "small child") would be confident in predicting the rising of the sun next day, they could also be said, to that extent to be able to explain it? - having some, albeit very elementary, grasp of the "underlying process" (Coddington, p. 5) that is involved?) But it seems that Coddington himself is not quite convinced by the objection either, for he goes on to write: "it may be that, at the deepest level of analysis, the distinction cannot be made between de facto regularities and causal processes: what we regard as causal processes may simply be de facto regularities of sufficient familiarity". However, he adds: "But at the level of analysis appropriate to the present discussion, the distinction seems sustainable, and, if any sense is to be made of the concept of explanation, indispensable" (p. 7). But why should a relatively superficial level of analysis be all that is "appropriate" for "the present discussion"? - a discussion presented as one that "has implications for the conflict between operationalism and realism", in which Coddington sees Friedman's view as "compatible with operationalism and the predictive but not the explanatory value of theories" (Friedman, of course, being held to take "the predictive performance of a theory as the overriding criterion of its acceptability"). See also note 23. (Some other points in Coddington's paper are given a more favourable reception in Chapters 4 and 5).

26. Again, the views of well-known philosophers are of course not sacrosanct (see note 25). In this case, the authority of Popper is

challenged by that of Kuhn. Kuhn takes a very different view of the significance of counter-instances, writing (1962, p. 80) "there are, I think, only two alternatives: either no scientific theory ever confronts a counter-instance, or all such theories confront counter-instances at all times".

As before (note 25), my general claim is only that Friedman's position is less parochial both in scope and in appeal than Wong appears to suppose. But any support for Friedman's views that is implicit in the writings of Popper is especially significant in this context, because Wong professes hostility to Friedman's position but draws heavily on Popper in developing his own.

(In claiming that some of Friedman's individual views have some respectability, I do not mean to suggest that his views as a whole must accord with a single, broad tradition in philosophy which is the respectable one. "Respectability" in this sense need not, for instance, coincide with "conventionalism" in Boland's (see note 4) (see, however, note 30 too).)

27. The moderate interpretation does run into problems with some of the examples Friedman gives, which it cannot readily accommodate; but it will be argued in Chapters 4 and 5 that the extreme interpretation would meet with similar problems too. (See also note 29).
28. The idea that Friedman is careless in "The Methodology" has appealed to some critics; but usually only as a possible explanation of some single statement, which alone is recognised as "errant". (See note 18).

29. One possibility that he might perhaps have drawn attention to is that "The Methodology" may not have been written expressly to express Friedman's methodological view but instead to support a particular group of economic theories. Were this so, then what will still appear as aberrations when Friedman is taken to be an instrumentalist (in Wong's narrow sense) need not after all be construed as unwitting ones but can be conceived as instead being deliberate. Sensing that even his, let us suppose, extreme instrumentalist views would not be strong enough to support his favoured economic theories, Friedman might have intermixed them in the essay with moderating statements, perhaps to confuse the scent and perhaps (to mix metaphors) to sugar the pill that non-instrumentalists too would be being asked to swallow. Such an interpretation (a far cry from the account Wong actually offers) would approach quite nearly the interpretation of "The Methodology" to be given in Chapters 4 and 5. But in these circumstances, such extreme views on method as Friedman might privately hold surely could not be attributed to him confidently without evidence that is independent of "The Methodology", where reservations are made; and it would not be reasonable, I think, to attack him as Wong's extreme instrumentalist on the basis of this essay alone. See note 27; and Chapters 4 and 5.

30. Wong writes of Friedman criticising, as it were inevitably, "from an instrumentalist point of view" (p. 318); and this makes me wonder whether he may have the Kuhnian account of incommensur-



able paradigms in mind here. Kuhn writes (p. 93): "When paradigms enter, as they must, into a debate about paradigm choice, their role is necessarily circular. Each group uses its own paradigm to argue in that paradigm's defense. The resulting circularity does not, of course, make the arguments wrong or even ineffectual ... Yet, whatever its force, the status of the circular argument is only that of persuasion. It cannot be made logically or even probabilistically compelling for those who refuse to step into the circle". But of course by no means all unresolved disputes are debates about paradigm choice; and this one doesn't seem to me to be: couldn't it be said that, whatever philosophical differences they profess, still both Friedman and Samuelson are practitioners of, and apologists for, the same (very broad) tradition in economics, practitioners that is of, albeit now perhaps near crisis, "normal science" in this field? (See notes 4, 26, 49 and 50; and Chapter 6). Nor is it clear that any differences there may be between Friedman and Samuelson here must stem from those differences in political persuasion or "fundamental differences in basic values" about which "men can ultimately only fight" (Friedman, p. 5) (if indeed such differences there be) (see the end of Chapter 4, and Chapter 6). Thus, even if there are disputes in which some sort of "stalemate" situation is inevitable, it remains to be shown that this is one of them. (See also note 35.)

Wong surely is a prisoner of the narrowness of his labels here. For, supposing that Friedman and Samuelson were respectively instrumentalist and descriptivist in some sense, there is still

some support in the literature for the ideas that instrumentalist and descriptivist positions may not always be fundamentally opposed and that, where they are, some issues may tell decisively between them. Popper holds that instrumentalism, on his main definition (which seems to be fairly close to Wong's) is opposed to the Galilean view that "theories are ... descriptions of the world"; though, in contrast with what I suspect would be Wong's view ("suspect", since the Galilean view is not the same as Wong's "descriptivism"), he argues that the struggle between the views so defined is one in which "there is much at stake" (1963, p. 101) and in which there are clear grounds, which presumably could be pointed out to the instrumentalist, for taking a stand "with Galileo against instrumentalism" (p. 117). On the other hand, he also allows that it would be in keeping with an instrumentalist line of argument to arrive at a position (one he has "not so far encountered in the literature" (p. 109)) according to which the dispositional words used in theories may be counted as giving descriptions, but descriptions that "have nevertheless a purely instrumental function" (p. 110). And the argument leading to this position is one which, Popper says, "is extremely difficult, if not impossible, to criticize; for our whole question - whether science is descriptive or instrumental - is here exposed as a pseudo-problem" (p. 109). Then Nagel (who characterises instrumentalist and descriptivist views as considerably more permissive than on Wong's account) points out that those who differ over whether theories can meaningfully be said to be true or false "frequently disagree neither on matters

falling into the province of experimental inquiry nor on points of formal logic nor on the facts of scientific procedure" (1961, p. 141). On Nagel's account, the opposition between the two positions would be more apparent than real, with each able to meet "the prima facie difficulties it faces"; whereas Wong seems to suggest that the opposition between the positions as he sees them is real and inevitable, but that each position is wrong. (Nagel does say that, with the positions he is dealing with often divided by "in part, loyalties to different intellectual traditions, in part inarbitrable preferences concerning the appropriate way of accommodating our language to the generally admitted facts", controversy "can be prolonged indefinitely". But he adds "the obvious moral" that, when the positions are stated circumspectly, "the question as to which of them is the 'correct position' has only terminological interest" (1961, p. 141)). See also notes 4, 5, 8, 17 and 48.

31. For the Theorem seems to ask us to consider a theory quite in general, in speaking of "all consequences"; and Friedman holds that if we do consider a theory without reference to any particular "use to which the [theory] is to be put" (Friedman, p. 26), then it becomes unclear what is to count as a consequence, the very distinction between assumptions and consequences becoming blurred. See also note 32.
32. Probably Samuelson would not be alone in taking the phrase to mean this. Of course such a reading does involve stretching the meaning of the term "consequences" to include even so-called

"assumptions", in so far as the latter are also implied by the theory; but the term can bear this technical sense. (Wong's point about differences over the forms of theories desired for practical purposes would then only be relevant at a later stage - the discussion of the importance of the, then valid, Theorem about necessary theoretical relationships).

Alternatively, Friedman might reject the Theorem as incoherent. Or he might argue that, when the term "consequences" does have a clear reference (i.e. in his terms, when the use to which the theory is to be put is known), "all consequences" comes to the same as just the consequences specific to (relevant in) that use, and that then, on his account of the uses of theories, the Theorem is false. (Here Samuelson might dispute whether these uses are desirable - a question that could still, however, be discussable). But this final possibility, which could conceivably be what Wong has in mind, involves, as I think Friedman would agree, a somewhat esoteric reading of the Theorem, with this time perhaps drastic confinement to the meaning of the word "all".

33. I wonder whether Wong may have here confused a claim that the assumption set's truth value is irrelevant to the acceptability of a theory with an, as it were higher order, view that any objections that might be raised (any arguments to the effect that truth of assumptions is relevant after all) are likewise irrelevant to the acceptability of the claim. (See also note 39.)

34. Virtually, if not quite all, Wong does suggest that the way in which

Samuelson has misinterpreted one point might be brought out in the defence (but see page 3.15). But a further objection that Wong permits his instrumentalist defender to make - that even if "the part ... of the assumption set corresponding to the invalid part of the consequence set" could be eliminated, still "it may be required for the generation of the valid predictions" (p. 316) - surely comes to little more than reassertion unless we are told more about what is meant by "required". If it means merely "useful" or "convenient", then doesn't the objection need expansion if it is to have any weight? Any circumstances that Wong's instrumentalists might appeal to here, however, as ones in which the retention of this part of the assumption set could indeed be convenient (see, e.g., Friedman, pp. 17 and 18), will surely introduce factors that would qualify to some extent the "single-mindedness" of their pursuit of immediate predictive success. On the other hand, the difficulty of identifying this "corresponding part" of the assumption set having been waived here, surely this part of the set, which presumably harbours its own invalidity, cannot be necessary for the generation of valid predictions? (See the end of Chapter 4).

Why doesn't Wong permit his instrumentalists to raise here themselves the difficulty he is later to raise independently (p. 318) - the Quinean point that it may not always be possible to single out one assumption in an assumption set as the "guilty party"? (a point that does, I think, have some force against the Corollary; though, on possibilities for sometimes detecting the "culprit", see Popper, 1963, p. 112). This is a difficulty which, it seems to me,

the instrumentalists are in a better position to highlight than is Wong; for, since he is to stipulate that the explanans of a theory "must not be known to be false", mayn't he be putting himself into the embarrassing position of having to jettison whole sets of assumptions because it is known they are somewhere false (though perhaps in the slightest way) but it isn't known where? See note 13 and page 3.14.

35. Dispute over "The Methodology" has indeed sometimes reduced to a similarly sterile series of claim and counter-claim, without fruitful exchanges (whence in part the Mystery? see page 3.9, and Chapters 2 and 4); but surely it need not have done (see note 30). And encouraging the series to continue further does not seem to me to be a forward step: Wong's "Friedman-type defense" (p. 316) carries the suggestion that no more powerful or productive answer to the charges (and in particular none of the elements in Wong's own "independent" critique) are likely to be available to Friedman.

Of course, reasserting those specific parts of Friedman's case that critics have overlooked in making their criticisms would be an important part of any answer to them; but, though Samuelson may indeed have been guilty of some neglect, the reassertive answer to him that Wong suggests is surely not one of the appropriate character. It seems to me (though not, I think, to Wong: see note 4) that in the "Friedman-Samuelson dispute" there is scope for reaching a far better understanding between disputants

than there has so far been (on which points, if any, are genuinely ones on which these disputants must agree to differ) and indeed scope for a fair measure of agreement too (see also note 50).

36. See note 20. Although Wong does appear to present Klappholz and Agassi's critical claims as ones that are true of instrumentalism without necessarily being sufficient to denigrate it, there is some confusion about Wong's own attitude here. (I suspect that, when he does eventually find fault with instrumentalism, he may be doing so on much the same grounds. See note 48).
37. Wong does, of course, spurn instrumentalism as "anti-intellectual" (see page 3.18). But the basis on which he actually makes this rejection seems, as I shall argue below, extraordinarily weak. Here, on the question of explanatory power, instrumentalist theories do, according to him, comply with a key requirement of going "beyond pure description" (p. 324); and instrumentalism appears to blot its copy-book in his eyes only by not noticing that they have done so (the "disregard" he laments). However, his instrumentalists could in principle accept theories which have known false assumptions, on all fours with theories which haven't; whereas holders of Wong's explanatory view could not; and so it is puzzling that Wong does not claim to have defeated instrumentalism in a very "similar fashion" to that used to "defeat" descriptivism. (Could it be that theories with known false assumptions are not, after all, to be rejected by Wong, since he too could be willing to use them for some practical purposes? (See note 19). Even if

he were willing to do so (and he does not canvass this possibility himself), he is still, in contrast with his instrumentalists, committed to regarding such theories as in principle having a different status from ones whose assumptions may be true).

If known false assumptions are necessarily inferior ones on Wong's explanatory view of theories, but are not necessarily inferior according to his instrumentalists, shouldn't Wong see this as a "substantive issue" in the dispute?

38. In his, not always adequately supported, attacks on Samuelson's position, Wong is outspoken: among many examples are "we have now shown that there is not much substance to Samuelson's critique from an instrumentalist point of view" (p. 318); "it was shown that Samuelson's presentation of the relationship between a theory and its assumptions and consequences is wrong" (p. 318); "now a refutation of Samuelson's descriptivist methodology will be given" (p. 320). It seems improbable, then, that Wong holds back from claiming Friedman vanquished too merely from diffidence: as witness also the confidence of his introduction ("What then is the excuse to write yet another paper on methodology? The alternative to bad methodological discussion is not no methodological discussion" (p. 312)); and the terms in which he goes on to disparage the position he cannot defeat (see below).

39. Or perhaps from confusing Popper's statement that "instruments, even theories in so far as they are instruments, cannot be refuted" (Popper, 1963, p. 113) with the meta-claim that the instrumentalist

view cannot be refuted either (see also notes 33 and 49; note 30 too).

40. The sentence Wong quotes (about "'assumptions' that are wildly inaccurate descriptive representations of reality ..." and etc.) comes from this passage: the different page reference he gives is an error.
41. Admittedly, though, Friedman's way of putting the argument could be misleading: see Chapter 4. (On the particular phrase, "descriptively false", see also note 42 and Appendix B.)
42. Indeed, in Chapter 4 a good deal will be made of the idea that what is literally a falsehood could perhaps actually fulfil the role of summarising detailed truth (in which circumstances it might be misleading to think of omitting detail and including falsehood as entirely separate processes: see Appendix B).
43. It could, I suppose, be the case instead that Wong has recognised that a third interpretation, not so common in the literature, is possible (see note 42) but has actually then been so swayed by Friedman's argument for that third position that now he is chary of parading too prominently his previous conviction that known false assumptions should be excluded. But I don't think this is likely.
44. It is tempting to remark that, had Wong himself articulated this reply (anticipating the objection to his account), he might then have become more receptive to Friedman's remark that the validity of a theory (in "Friedman's sense") was not a sufficient criterion

on which to choose between theories.

Without some such additional argument, Wong would seem to have to withdraw his stipulation about truth and concede that, on this, the instrumentalists have been right. With such an argument, must he concede that Samuelson is right? Wong denies that the choice must be one between Samuelson and the instrumentalists; but, on the other hand, surely he must choose between rejecting known false assumptions and not rejecting them? Perhaps he could still argue, however, that Samuelson is right about what the outcome of the latter choice should be, but is right for the wrong reasons. (See note 4; but also note 50).

45. Wong may fancy that it will not do to criticise his instrumentalists for failing to live up to standards that they themselves would repudiate: if the sole interest his instrumentalists have (and acknowledge as worthy) is prediction, is it reasonable to criticise them for not serving the cause of explanation? But Wong's descriptivists were similarly interested in description rather than explanation; and they were taken to task for this. (See notes 30 and 37).
46. See note 17.
47. Wong does tell us that he takes the epithet to express "the spirit of Samuelson's F-Twist caricature". But since, according to Wong, Samuelson's F-Twist account is not only internally unsatisfactory but is also, apparently in almost every particular, a "misinterpretation of Friedman's methodology" (p. 316), I do not see just what "spirit" Wong can think is left.

For the adjective "intellectual", the concise Oxford Dictionary (more or less echoed by Webster) gives: "of, appealing to; requiring the exercise of intellect". Now surely forming any general doctrine about scientific method, instrumentalism not excepted, will require "the exercise of intellect"; as will putting even Wong's crude version of instrumentalism into practice (for instance, which predictions are to count will be a matter of judgment for Wong's instrumentalists, for they do not deal (even then, mechanically?) with a theory's "complete" consequence set). Then Wong is committed to claiming that his brand of instrumentalism does at least "appeal to the intellect" of Friedman; whilst Popper, to whom Wong refers us for discussion of instrumentalism, presents it as having "appealed to the intellect" of Bohr, Bridgman, Duhem, Eddington, Hertz, Kirchhoff, Mach, Poincaré, Schlick, Ryle and Wittgenstein (Popper, 1963, p. 99, n. 5, etc.). (Nagel has the instrumentalism of his definition as appealing also to the intellects of Peirce, Ramsay, Dewey, Watson and Toulmin). Aren't any of these "intellects" to count?

48. Given his reliance on Popper, it indeed seems fairly likely that Wong's intention here (though see note 49) is to echo Popper's indictment of instrumentalism as being, despite what he sees as attractions, "as obscurantist a philosophy as essentialism" (1963, p. 113). Popper does attempt to justify his charge. Taking the instrumentalists' attitude to be "one of complacency at the success of applications", he argues that their interpretation will "be unable

to account for real tests, which are attempted refutations, and will not get beyond the assertion that different theories have different ranges of application. But then it cannot possibly account for scientific progress" (1963, p. 113). He contrasts the prediction of events of a kind which is known (e.g. the prediction of eclipses) with the prediction of "new kinds of events" (discoveries) (p. 117), saying that instrumentalism can only account for the former, and that it goes against the scientific spirit of discovery - the attempt to explain the known by the unknown and thus to extend the realm of the known.

According to Popper, instrumentalism is out of keeping with the Greek tradition of rational criticism in accordance with which scientists have created myths or conjectures, theories "which are in striking contrast to the everyday world of common experience, yet able to explain some aspects of this world of common experience" (p. 102).

These criticisms of instrumentalism do not command universal assent; and Nagel in particular has questioned their applicability. But if Wong is taking them to be powerful (and so as unquestionably decisive against those he would call instrumentalists in natural science?), surely they still would not justify spurning Friedman's arguments on methodology as "anti-intellectual", because

(a) we need to be told more about what constitutes a "real test" and a "discovery" in the social sciences, where Popper himself says that theories "are at best approximations" (see page 3.10);

(b) in his gestures towards a fuller view of scientific method,

Friedman doesn't seem unduly complacent about the past "success of applications" (e.g. "any theory is necessarily provisional and subject to change with the advance of knowledge" (p. 41); "the tentative state of knowledge that alone makes scientific activity meaningful" (p. 34); (but see also (d));

(c) he does seem to hint at a criterion of progress, when (pp. 33 and 34) he speaks of the increasing ability of science to reveal more "fundamental" and "simple" structures (see also Chapter 4); and he himself points out that he has not described the process of scientific discovery (recognising that "the construction of hypotheses is a creative act of inspiration, intuition, invention; its essence is the vision of something new in familiar material" (p. 43) - Grecian, this?);

(d) although the specific hypotheses he discusses can indeed scarcely be looked to for successful predictions of new kinds of events, so that there may appear to be, and perhaps even be, some complacency here (see Chapters 4 and 5), still he is after all arguing against those who would thoughtlessly discard such hypotheses altogether, out of hand; and in so far as his claim is only that theories known to be imperfect can still be used to advantage, the more so in the absence of worthy rivals, Popper would agree with him (see note 19).

Even if the "anti-intellectual" charge does have meaning, then, as an oblique reference to Popper's charge of "obscurantism", it is a charge against Friedman that need not stick. (Would such a charge against Friedman differ much from the criticisms

Klappholz and Agassi make, either? See note 36).

49. His next sentence is: "instrumentalism provides a self-justification for its methods and its methodological choices." Does this add anything? - I am not sure what the statement means. If it is about the circularity involved in defending an alleged paradigm, then it isn't clear that the statement is true (see note 30: mightn't the relevant paradigm, within which criticism must take place, be the, only identical if instrumentalism is completely justified, one set by scientific method itself?); and, if it were true, surely any rival methodology would be in the very same boat. Or is the statement again intended to echo Popper's attack (on the grounds of complacency (see note 48), and the grounds that "the instrumentalist view may be used ad hoc for rescuing a ... theory which is threatened by contradictions" (Popper, 1963, p. 113))? - if so, there seems to be some confusion between justification (of practice, by a doctrine on method) and self-justification (of one of these, by itself). Wong does offer quotations from Johnson (1971, p. 13) in illustration of the "self-justification" statement, but their relationship to it is not clear. The specific plaint Johnson makes against the methodology of positive economics (that it necessarily repudiates interest in explanation) is not, I think, enough to back Wong's position against Friedman here (see page 3.9; note 37; and also Chapter 4).
50. For, notwithstanding his criticism of Friedman in 1963 (which I am tempted to describe as an S-Twist), Samuelson himself has

elsewhere shown, both before and since, surprising tolerance of assumptions that seem to fit the Friedman mould. Loud echoes of "The Methodology" are present in Samuelson (1962) (an article itself to become notorious) where, having claimed some value for truly "heroic abstraction", he writes (p. 194): "What I propose to do here is to show that a new concept, the "Surrogate Production Function", can provide some rationalisation for the validity of the simple J.B. Clark parables which pretend there is a single thing called "capital" that can be put into a single production function and along with labour will produce total output (of a homogeneous good or of some desired market-basket of goods) ... I shall use the new tools of the Surrogate Production Function ([note :] one might call this the As If Production Function) and Surrogate Capital to show how we can sometimes predict exactly how certain quite complicated heterogeneous capital models will behave by treating them as if they had come from a simple generating production function (even when we know that they did not really come from such a function)." Then in 1965, after giving explicit recognition to the value of theories that are simple or that link well with other accepted theories, Samuelson goes on to admit that there could be a situation where one should accept a theory some of whose consequences have been refuted (a situation in which the discrepancies are agreed to be unimportant; and in which no alternative theory that would share only the valid consequences of the theory in question is available). Most recently comes the Nobel Prize

lecture of 1970 (A.E.R., 1972) (based in part on an earlier paper in Lerner (ed., 1965)). Here Samuelson speaks with approval of Mach and of "more economical" descriptions of nature. And he uses examples from physics - examples with a familiar sound: "As I shall discuss in connection with the role of maximum principles in natural science, the plumb-line trajectory of a falling apple and the elliptical orbit of a wandering planet may be capable of being described by the optimising solution for a specifiable programming problem. But no one will be tempted to fall into a reverse version of the Pathetic Fallacy and attribute to the apple or the planet freedom of choice and consciously deliberative minimising. Nonetheless, to say 'Galileo's ball rolls down the inclined plane as if to minimise the integral of action or to minimise Hamilton's integral' does prove to be useful to the observing physicists, eager to formulate predictable uniformities of nature" (1972, p. 250). This passage might surely almost have been Friedman's own. But no mention is made of him.

Chapter 4THE WHITE, THE GREY AND THE BLACK

It can generally go without saying that reaching a fair judgment on what a man says requires first a knowledge of what he does say. Yet much of the hottest debate in the Friedman Affair has concerned propositions that it is arguable Friedman is not committed to; whilst some objections raised, as though for the first time, are ones he had anticipated and tried to meet in 1953.

It would be possible, though tedious, to document in detail this history of neglect of Friedman's statements,¹ with the scorning of debate by Friedman and some of his followers that has been associated with it; but behind the history would still lie the question why there should have been so much neglect. Its persistence and extent point to more than simple slackness. One influential factor may well have been the convenience of the bastard versions of "The Methodology" for "justifying work that produced apparently surprising results without feeling obliged to explain just why they occurred" (Johnson, 1971, p. 13);² but by no means all contributors to this methodological debate have had private empirical axes to grind.^{3, 4} The key to the misunderstandings seems to me to lie rather in the fact that many critics of Friedman's essay, feeling sure it is somewhere at fault, have sought the error in the wrong place.

For though "The Methodology" is written with such easy elegance, the very smoothness masks a tension between two themes. I want to

suggest that recurrent misreadings of the essay have resulted from attempts to criticise it as a study in the large of methodology, promised in the title, without taking into account the rather special nature of Friedman's brand of the positive economics billed there too.⁵ Friedman's underlying aim in "The Methodology" is not after all, I think, to give a general account of correct scientific method, merely illustrating the story from his own field of interest: his concern is with establishing a particular theory of the firm (and other economic theory of similar origin),⁶ rather than with methodology as such.⁷ The nature of the economic theory Friedman champions then explains why so much of his essay concentrates on exposing the naivety of those who reject "unrealistic", and indeed false, assumptions out of hand;⁸ and his emphasis on this need not be taken to show that he sees in falsehood an end to be pursued for its own sake and even to be given pride of place in his whole methodological system. But if the aim of "The Methodology" is indeed of so specific a character, then some familiar types of objection to the argument are conspicuously incomplete; and those who have made them, however correct in their basic conviction that some move in the essay would prove to be objectionable, have been much too hasty in claiming already to have uncovered the move that is.

On the interpretation that I am suggesting, Friedman would lay special stress on just those principles of method that might then seem to underwrite the specific case that he has at heart. Thus, the theme that standard scientific practice rightly involves being selective and telling less than the whole truth would be specially highlighted where it

verges on licensing untruth too, largely because falsehoods enter into the case Friedman is concerned with. But, in terms of the lie-telling analogy, if Friedman is arguing for "telling fibs" only in certain situations, then those criticisms are indecisive that assume he advocates this "evil" universally, never recognising the value of what is good. Some critics take his remarks about lying out of context and there condemn them, conflating pleas in defence of this lie or that with exhortations to fib all the time; whilst others take him to task for failing to extol truth-telling, neglecting the fact that he may be concerned with those special circumstances in which, his whole point is, the literal truth need not be told. What both groups leave out of account is the question whether Friedman's lies are white. Whether you call them white or not, yet others might say, they are still lies; and the damage done to the practice of being honest by allowing exceptions to the truth-telling rule will far outweigh any direct good achieved by particular lies. From this point of view, even allegedly "white" lies will come out on balance as black. But this attack too is weakened by failure to refer in detail to Friedman's particular cases; for if the situations in which he advocates "lying" are truly distinctive, so that permitted exceptions to the rule are well defined, then the rule may retain its force: it will not be easy for everyone to claim that his case is a "special" one too.

At this point, the critics might very well protest that Friedman's aim appears to be less narrow than I have suggested, his case on truth requirements for assumptions being presented at the outset as a general one. But, in the first place, some magnification, by means of tone, of

the general scope of an essay designed in the main to further a specific cause that is a controversial one might give testimony to skill in persuasion, rather than to the nature of the writer's prime objective;⁹ and, in the second, do the explicit generalities there are in "The Methodology" really favour falsehood in general?¹⁰ Certainly, Friedman writes (p. 14) that "truly important and significant hypotheses will be found to have 'assumptions' that are wildly inaccurate representations of reality, and, in general, the more significant the theory, the more unrealistic the assumptions (in this sense)". But just what is this sense? He continues by saying that an "unrealistic" or "descriptively false" hypothesis "abstracts the common and crucial elements from the mass of complex and detailed circumstances surrounding the phenomena to be explained and permits valid predictions on the basis of them alone ... it take account of, and accounts for, none of the many other attendant circumstances, since its very success shows them to be irrelevant for the phenomena to be explained". This seems to admit the possibility, as some commentators (Wong amongst them) have allowed, that assumptions might qualify as "descriptively false" for Friedman just through failing to catalogue the truth exhaustively, without actually being false as well; and, more importantly, it seems also to show that, where the "descriptively false" does involve untruth, the case Friedman emphasises (and Wong seems to neglect) - still he is not saying, as some critics take him to be, that any old falsehood will do.¹¹ Of course, to use the single phrase "descriptively false" to cover both these situations is to blur this distinction between truth and falsehood; but Friedman's point here may be that there is a rationale for sometimes doing so - a rationale

which constitutes the justification for accepting¹² some assumptions that are false. For his highly effective argument that we do not explicitly include in an assumption set every single known truth about the phenomena concerned does draw attention to our "revealed preference" for simplicity; and simplicity, given full rein, can perhaps ride roughshod over truth. Some well-established processes of simplification and abstraction can lead to statements that, though summarising much of the truth, might not themselves be held literally true.¹³ Hence Friedman could make the claim that, just as any attempt to mirror the truth in its entirety would add valueless clumsy complication to an assumption set, so also does a requirement that assumptions must be true - because it excludes, without even giving them fair trial first, a whole realm of "respectable" abstractions and approximations. Seen in this light, his thesis would be that stating the relevant truth about some phenomenon simply, for a particular explanatory or predictive purpose, is sometimes (he might say, fairly often in our present state of knowledge¹⁴) best achieved by means of literal falsehoods; and he would be promoting fibbing only as an efficient means to a good end.¹⁵ But any false assumptions admitted accordingly ought to be "sufficiently good approximations for the purpose in hand" (Friedman, p. 15), approximations, that is, of the truth; whilst the predictive test of such sufficiency appears, on this basis, not as a test that will be adequate taken in total isolation, but rather as a control on a process of simplification that has (apparent) reality as its starting-point.¹⁶ Such a justification for waiving the truth requirement, then, is only an aspect of

the wider case for framing simple theories that "abstract essential features of complex reality" (Friedman, p. 7)¹⁷; and, as such, it can justify only a very special group of lies - those whose "whiteness" consists in their epitomising truth.

However, as Friedman himself acknowledges, but his devotees often forget, "criticisms may miss the target, yet there may be a target for criticism" (Friedman, p. 41). I believe that there is such a target in "The Methodology", and that the failure of so many critics to hit it decisively may be more than a mere coincidence. Friedman's explicit claims on methodology are open to a moderate interpretation on which his attackers have made next to no impression¹⁸: even on the question of "fibbing", he can be seen as arguing for treating "lies" as white only if they contribute to simplicity, by abstracting away only from trivial details of reality, whilst yet yielding valid predictions.^{19,20} But, if my view of Friedman's main aim as yet more specific is correct, these muted methodological precepts, even if accepted, could not bring him success in themselves. To establish his favoured theory of the firm, he would still need to show, firstly, that his criteria for whiteness are precise enough to distinguish black from white effectively²¹, and secondly, that they are actually fulfilled by the falsehoods in his theory. Friedman's main efforts, however, go to routing old enemies instead. He makes much of the fact, rashly overlooked in some past arguments for abandoning altogether the theory of the firm based on the assumptions that Friedman recommends²², that to exhibit the falsity of a theory's assumptions is not ipso facto to dismiss its conclusions²³; but he keeps

studiously quiet about the general possibilities for fair dismissal. At the same time, with what seems like masterly cunning²⁴, he seeks to shift the burden of proof over his theory of the firm onto his opponents, putting on them the unexpected onus of showing that the excluded facts they champion have not only indeed been omitted or denied, but are also important and relevant, so that including them will add more than just, howsoever true, clutter. These moves are so skilful that commentators, fussing over them, might, perhaps forgivably, forget to consider whether the proof could be supplied. But in the last analysis, the crucial question for the theory is whether the excluded facts do matter, rather than whether their promoters, spotting at length Friedman's failure to show that they don't, will recover sufficiently to show that they do.

Then what I am taking to be the, very specific, issue is undoubtedly further obscured in "The Methodology" by the whiff there may be of methodological principles that are not moderate, of arguments for falsehood that depend on no appeal to special cases - a whiff all the more perplexing since it does not originate in substantial commitments to the extreme, in the text. Friedman's discreet silence about the drawbacks of using false assumptions may be part of his strategy against the inveterate champions of "realistic" ones; and it is, I have argued, compatible with a moderate methodology that allows the virtue of keeping contact with truth. But nonetheless, to stress a possible role for false assumptions without making even passing reference to their limitations too may be to court, for whatever strategic ends, appearing misguided and extreme.²⁵ It is understandable that thwarted critics do not feel

quite reassured when the seemingly extreme simply evaporates under their pressure, to become measured moderation instead. Similarly, Friedman's lack of explicitness about the rationale for adopting false assumptions is mysterious, especially as his stated position might be thought to stem from belief in a fallacious argument: the argument that, since the truth of a statement (about the subject-matter of a theory) is insufficient to justify its inclusion in the assumption set, mere parity of treatment demands that the falsity of another statement should not be sufficient to justify exclusion. This is fallacious because the insufficiency of truth as a lone criterion for assumption choice does not imply its irrelevance (assumptions' truth might, after all, still be necessary), and consequently treating truth as irrelevant is not required for consistency; and one wonders why Friedman does not, to reduce any risk of such an argument being rashly attributed to him, give more detailed guidance on the basis for his position - a position supported by the far more appealing argument that, when assumptions are serving to simplify the welter of minor details of (apparent) reality²⁶, their literal truth may not be necessary after all. Moreover, as the text stands, even the latter argument might be thought to lend itself to an extreme use; for though Friedman's false assumptions do have to be "sufficiently good approximations" of the truth, he adds (p. 15) that we can find out "whether they are ... only by seeing whether the theory works, which means whether it yields sufficiently accurate predictions". Here Wong's view that predictive performance is a sufficient test of a theory for Friedman might well appear to rear its head once more, with ability to "work" being Friedman's sole criterion for the whiteness of lies²⁷.

Yet Friedman, having earlier stated clearly that predictive success is at most a necessary requirement, could have avoided any confusion at this point by stressing that he is speaking, in effect, of the predictive test as a control (for its being, for him, the only possible test of whether approximations are "sufficiently good" need not mean it is a test of what is an approximation too - this not being a question of truth alone, and so not decidable by direct "tests" of truth): he could, that is, have emphasised the fact that all the candidates for testing should already be simplifications of the truth, drawn in some conventional way.²⁸ Finally, one might wonder on grounds of style too how far Friedman's essay is truly intended to strike a moderate chord. At times, his tone appears temperate enough, and his manner ingenuous, as he leads us on by soothingly steady steps of reasoning; but at others, the prose becomes polemic and provocative as, to opponents of the beloved business theory, he expresses defiance that may border on contempt. Yet if his general methodology really is so moderate, and his point against opponents of the theory is so clear (being the point that, having disregarded matters of logical fact, they rely on an argument that is indisputably incomplete), why devote such impressive effort, to persuasion on the one hand and to denunciation on the other? These disparities between the potentially moderate substance of Friedman's general methodological statements and his melodramatic manner of making them have doubtless encouraged misunderstandings and fostered the Methodology Mystery.

But I believe that these riddles can be solved; and solved in such a way that any mere whiff of the extreme in Friedman's essay need not be taken to countermand the moderate view of much of his argument

about methodology there. On the interpretation I am suggesting, a plausible account can, I think, be given of why overtones that are extreme might yet be welcome in "The Methodology" - an account that allows for the difficulties Friedman meets in the essay in his attempt to realise a very specific aim, and that explains too why so many critics have suspected something was amiss without actually being able to detect it. Thus, it seems to me that some of the examples Friedman canvasses²⁹ can in practice only be defended on the basis of views more extreme than he ever officially avows in his preaching - views that, though vulnerable, cannot readily be pinned on him, Wong-fashion, as general claims in "The Methodology", especially since these views do not cover all the examples either.^{30, 31} And it seems to me too that, whether deliberately or unconsciously he alone can say, he also sets up a hue-and-cry in the best tradition of diversionary tactics.³² Here are his pursuers, seeking to censure his challenging yet guarded remarks about the requirements for assumptions and the general significance of "predictions" (but always taking it for granted that particular conclusions come up to the mark); or intent on faulting his insistent but blameless argument that a theory with false assumptions may yet have true conclusions; or trying to track down a fanatical general case for falsehood that always seems to prove a will-o'-the-wisp: all of them thinking they are hot on the scent of his methodology as such.³³ And all the time he is leading them away from a cool consideration of the facts in his central economics case.

Friedman is, in fact, far from forthcoming about the marked

differences between his specific examples, and about the various ways in which their assumptions, to qualify as white, are held to compensate for the limitations inherent in being false. For whatever their value in terms of simplicity, and despite their compatibility with true conclusions, it remains the case that false assumptions are not ideal: if they are preferred, this is only as a "second best".³⁴ For instance, unlike true ones, they carry no general guarantee that, barring mistakes in reasoning from them, their conclusions will be true: they may have conclusions that are true but won't necessarily do so.³⁵ Friedman's point that conclusions drawn from false assumptions can be true is important against naive insistence on eschewing false assumptions whatever the price; but he neglects to say that perfect reasoning from false assumptions can only be expected to lead unerringly to true conclusions in conditions that are highly restrictive. Assumptions that are false, it will be suggested below, can only be used reliably to predict what is already expected on other grounds to be true - when all the glory must by rights belong elsewhere. And it is in where the glory truly does belong, in the location of these independent grounds for confidence, that the divergencies between Friedman's various examples really become apparent. In what follows, I shall depart from "The Methodology" by dividing the class of false assumptions, perhaps somewhat crudely³⁶, into three groups.

The falsity of "as if" assumptions that, in particular uses,³⁷ come into the first group (assumptions-A) stems from their elimination from the theoretical picture of relatively minor factors (thus permitting concentration on the crucial ones) that are at work in actuality. Friedman evidently believes (I think, mistakenly) that most, if not all, the false-

hoods he is concerned with come into this category. Thus he writes (p. 40) that "a meaningful scientific hypothesis or theory typically asserts that certain forces are, and other forces are not, important in understanding a particular class of phenomena. It is frequently convenient to present such a hypothesis by stating that the phenomena it is desired to predict behave in the world of observation as if they occurred in a hypothetical and highly simplified world containing only the forces that the hypothesis asserts to be important". Now it might be natural to feel confident that assumptions-A, despite their falsity, imply certain conclusions that will be consistently borne out at least approximately by the evidence, because assumptions-A have the merit of paying some court to the true grounds for any such success. There can be confidence in their predictive performance because, though false, they yet epitomise the truth, by isolating key operative causal factors that are working to effect broadly the implied result. Similarly, assumptions-A can have explanatory significance, since they express in stylised fashion the actual mechanisms at work. The explanatory and predictive value of assumptions-A, then, depends on their special relationship with the truth: they are derived by certain well-entrenched methods of abstraction from the real underlying conditions, so that the nature of their departure from the solid foundation of reality is known; and it is this link with a firm foundation that allows us to use them with some freedom. At the same time, some warning signals are also built into assumptions-A: knowing what is assumed away in the expectation of its influence being small is some preparation for recognising a change

to circumstances where its influence may be greater, for it is already a realisation that the influence is there. Of course, it may only be possible to tell whether influences assumed to be negligible are really too small to matter^t by putting theories so assuming to the test - as Friedman, playing one of his trump cards, adroitly argues. But he does not point out that, by considering the relation between the assumptions and the truth, it should be possible, in the context of assumptions-A, to gain an idea, in advance, of what the influences are that are being tested.³⁸

The situation is different with assumptions-B, a group that Friedman also admits in practice, though perhaps only faute de mieux.³⁹ Assumptions-B, though false, nonetheless do imply conclusions that prove persistently to be true (or very nearly true); but, in contrast with assumptions-A, they have nothing to do with the fact of their conclusions' truth. Assumptions-B, then, have true (or nearly true) conclusions, but lack the special relationship with the truth that characterises assumptions-A. And so their success may appear miraculous since, unlike assumptions-A, they give no hint of the actual mechanisms on which it relies. Yet assumptions-B depend on a methodological conjuring trick by Friedman, rather than on any miracle. Whereas assumptions-A are related in certain well-entrenched ways to statements of what actually is the case, so that what they assume is a close approximation to or abstraction from the real causally operative state of affairs, assumptions-B by contrast do not correspond by the recognised abstractive routes to anything in nature, instead relating to statements

of actual causal conditions in the spurious fashion of sharing the same implications. That assumptions-B have true implications is a theoretical windfall, possible because of the operation of causal mechanisms that they make no acknowledgement of: they are parasitic performers that, in contrast with assumptions-A, masquerade as if they were not.⁴⁰ Theories built on assumptions-B, then, only pretend to truly independent predictive power and merely create an illusion of offering any explanation themselves. Moreover, such theories harbour dangers, since bad break-downs in the underlying causally effective conditions need have no bearing on assumptions-B themselves, though jeopardising their conclusions' truth. On the other hand, so long as their implications do happen to be true, assumptions-B could conceivably have a role, perhaps pending the accurate statement of preferable ones, or perhaps merely as embroidery on descriptions of events, embellishing accounts of the already known.⁴¹

Might Friedman here dispute the basis for distinguishing at all between assumptions A and B, arguing (as many commentators would have him do) that both groups are equally acceptable since they score success with their conclusions, and that the greater descriptive inaccuracy of the assumptions-B themselves is merely an unimportant matter of degree? And indeed, is the distinction adequately based? Friedman's "crucial element" (p. 14) and "important force" (p. 40) passages suggest he there has only assumptions-A in mind; and I do not think his argument precludes him from seeing the two groups as distinct.⁴² And surely the distinction between groups A and B is one

that can in principle be made. Admittedly, the term "well-entrenched" that is used in drawing the distinction is, though familiar in another philosophical context, conspicuously vague here⁴³; but vagueness in marking the distinction may be a sign that methods of simplification will need to be investigated in greater detail⁴⁴ before there can be any more exactness, rather than that there is nothing to try to be more exact about. It does seem to me that assumptions that, for instance, postulate a theoretical limit to an existing series of observations

will form a group distinct from

assumptions-B - assumptions which, rather than just allowing the effects of an operative and powerful force to be considered in isolation from other forces in a hypothetical simulation model that is set up, serve instead to introduce a hypothetical motive force.⁴⁵ If assumptions-A can qualify as "white", surely assumptions-B can at best be counted only as "grey".

However, in any case it is the distinction between both these groups of assumptions and a third that will be most critical in what is to follow.⁴⁶ This third group is the group of those false assumptions (assumptions-C) which do not abstract by well-entrenched routes from the statements of the actual causal conditions, as assumptions-A do, and which also lack the background mechanism to bring about the truth of their implications that assumptions-B rely on. Assumptions-C, then, are the residual group - but a very important one. Their key distinguishing mark is that, in particular uses, their falsity does carry with it denial of truth (and even of near truth) to their conclusions⁴⁷; so that even

attempts to use them in a decorative role will fail. Merely sufficient assumptions that are false will fit into the C category, then, whenever it is the case that no other assumption whose truth would be sufficient for the truth, or near-truth, of the conclusion in question is true either; and of course false "as if" assumptions whose truth is necessary for the truth (or near truth) of a particular conclusion will always count as assumptions-C, when they are used to "predict" it. And assumptions-C are "black". Now this might have been a fruitful avenue for Friedman's critics to follow; for if the truth of any of the assumptions in his examples could be shown to be necessary for the truth (or near truth) of the conclusions there being drawn, then the agreed falsity of that assumption would matter, against Friedman's claims.⁴⁸ But critics have not taken this path, and the Mystery muddles on - a measure of the skill with which Friedman has obscured the issue here, if any of his assumptions is indeed an assumption-C. Some may, of course, have felt that here the issue really lay, but refrained from probing too deeply because they shared Friedman's commitment to the value system of which the economics at the heart of his essay is often held to be an integral part;⁴⁹ but I doubt if they are many. Friedman himself claims, moreover, that in many cases positive science can be distinguished "sharply" (p. 7) from normative science, and that his essay is "concerned primarily with certain methodological problems that arise in constructing ... 'distinct positive science'" (p. 3); and so it would be in keeping with "The Methodology" itself to judge it with Joan Robinson's view in mind - the view (1973, p. 122) that "differences of opinion there will always be where political issues are involved: these are differences of

judgment and of moral values. They should not affect logical analysis".⁵⁰

Which, then, of Friedman's "as if" assumptions fit into which of the categories A, B and C? It is time to move on to another chapter.

Appendix IVFootnotes to Chapter 4

1. The survey of the literature given in Appendix A provides further details of this neglect, in a summary form.
2. How are Johnson's "results" related to what Friedman would be willing to call "predictions"; and how does the "work" he refers to compare with Friedman's "hypotheses"? Johnson holds (and Wong expresses some agreement: see Chapter 3, note 49) that "the demand for clarification of the mechanism by which the results can be explained is contrary to the methodology of positive economics"; but I do not think it is necessarily contrary to the arguments presented in Friedman's "Methodology" (see Chapter 3, note 25 and page 3.9). See below and Chapter 5, however, for a significant qualification to be made about some of the specific examples Friedman uses (on which, see also Chapter 3, note 27).
3. Friedman, I think has an axe of his own to grind - but not one that, by following Johnson's version of positive economic methodology, he could legitimately present as being ground. See below, and Chapter 5.
4. Similarly, though some may have taken a stand on "The Methodology" less because of its arguments than because of their own political persuasion, still I don't think this factor is sufficient to account for such widespread confusion. (See below).

5. See note 33.
6. See Chapter 6, note 12.
7. Some of the very many commentators have, of course, given parts of the interpretation to be suggested in what follows, without linking whatever part they give to the other parts in the pattern I shall suggest (see note 33). Those commentators who do in some way specially highlight Friedman's particular concern with economics here are in the minority; and even they rarely discuss his economic example in much detail. Among them are Nagel (1963), who notes that there is some tension between two themes; and Samuelson who, in the 1960s, ~~citing~~ Rotwein (1959), says that what he dubs the "F-Twist" seems significant for economics, rather than philosophy of science, being apparently designed to help the perfect competition, laissez-faire model and, "incidentally", the maximisation-of-profits hypothesis (how much difference does it make that Friedman deals in "expected returns" rather than "profits"? See Friedman, p. 21, n. 16; and chapter 5 below.) Rosenberg (1972) does give a fairly searching analysis of Friedman's specific business hypothesis; but I do not agree with his conclusions (see Chapter 5 and Appendix A). See also note 8.
8. I am sure that Klappholz and Agassi are right to recognise the significance in "The Methodology" of Friedman's desire to vindicate specific economic theories, writing, before going on to make some criticisms of the essay (see Chapter 3), that "the central thesis of Friedman's essay is that much criticism of economic

theory has been methodologically wrong-headed and misses the target, a thesis he easily establishes." However, I think that his central aim of pleading for specific theory goes rather beyond just rebutting its critics.

9. One might well tend to exaggerate the general value of one's case (by means of suggestion) I think, if one really had a somewhat personal goal at heart in arguing the case at all, even where the case unembellished could probably suffice to bring off one's aim. But I shall suggest below that there are additional reasons why Friedman might slip into heightened language, though pursuing a limited aim (I am not arguing, however, that Friedman can be seen to be deliberately deceitful (see note 32), and Chapter 5).
10. "General" is a term that seems to have two, on the face of it widely different, uses in the philosophy of science; and it seems to me that some confusion between them may have entered into the literature of the Friedman Affair. Is a "general claim" one that holds true without qualification; or is it one that "covers" a very wide range of phenomena (though perhaps not being literally true of every individual instance of them)? I think Boland probably intends "general" in the former sense when he speaks of the simplicity/generality trade-off (with, he says - in what may be an over-simple view: see below note 14; also Chapter 3, n. 50 - Samuelson opting for generality, Friedman for simplicity): a sense that may perhaps coincide with what others have meant by "realistic". On the other hand, in speaking of "general equilibrium theory", we seem to intend

"general" in the, apparently rather different sense of "all-embracing". But may such opposition as was suggested above between being strictly true and having breadth of scope, between accuracy and inclusiveness, be a false one? Mightn't a truly "general" claim have both features? I think Friedman might answer these questions in the affirmative. He speaks of "the extra accuracy" yielded by a "more general theory" (p. 18); and of "stating assumptions so as to bring out a relationship between superficially different hypotheses" being "a step in the direction of a more general hypothesis" (p. 29). And he explains that a theory counts as "more 'fruitful' the more precise the resulting prediction, the wider the area within which the theory yields predictions, and the more lines for further research it suggests" (p. 10) - evidently seeing these factors as being in harmony with one another. See note 14; and Appendix B.

11. See Chapter 3, pages 3.15 to 3.17.
12. On the meaning of "accept", see note 41.
13. See Appendix B.
14. There may be some suggestion in "The Methodology" that employing these, so to speak, "abstractive falsehoods" may be a temporary expedient, though perhaps a necessary one at this stage in the development of economic theory. Are there systematic reasons why true assumptions must forever be complicated? Friedman gives no explicit answer; but, in his optimism about the progress of science, he sometimes seems to suggest that the trade-off between

simplicity and "realism" may not be permanent and inevitable; that there are potential possibilities for arriving at assumptions that are both simple and true, potentialities not yet realised perhaps because economics is still an infant science. Thus, he criticises (pp. 33 and 34) Alexander's view that "economic phenomena are varied and complex, so any comprehensive theory of the business cycle that can apply closely to reality must be very complex", saying that "if a class of ^{'economic} phenomena appears varied and complex, it is, we must suppose, because we have no adequate theory to explain them." And he tells us that it is "a fundamental hypothesis of science ... that appearances are deceptive and that there is a way of looking at or interpreting or organising the evidence that will reveal superficially disconnected and diverse phenomena to be manifestations of a more fundamental and relatively simple structure". But if the "fundamental structure" of reality really is simple, then perhaps we may sometime arrive at assumptions that are both simple and true: "general" theories may then no longer be more costly to use; and artificial devices of abstraction, having outlived their usefulness, may wither away. (He also remarks (p. 26) that assumptions may be singled out as crucial on the grounds, *inter alia*, of "intuitive plausibility": assumptions may not everywhere have to be true, but it's good if they're next door to it). See notes 10 and 16, and Appendix B.

Phelps-Brown (1972), following Morgenstern, supports the view that economic science is at a relatively early stage in its development, whilst Worswick (1972) to some extent dissents. (All

seem to share in the belief that some progress is possible, but also believe, unlike Friedman one suspects, that recent developments have not represented much of an advance. See Chapter 6.) Coase shares in optimism that it may be possible to find some assumptions that are both "manageable" and realistic (see Appendix B).

15. Might falsehoods have a necessary role? See Appendix B.
16. Here and below, I leave aside the question how we know what "reality" is.
17. See also Chapter 9.
18. See Appendix A.
19. "Trivial", for a particular predictive (explanatory) purpose.
20. On the question of how far this squares with his examples, see below.
21. Might Friedman claim that no such distinction need be made? See p.4.14.
22. Friedman's business ⁱ"as if" hypothesis, though based on the same assumptions as the traditional theory of the firm, may be articulating them in a new way (see Chapter 5 and Appendix C); but sometimes "the theory" will be used below to refer to both.
23. For some attempted criticisms of points of logic in Friedman's case, see Appendix A.
24. See note 32.
25. See pages 4.3 and 4.4.

26. See note 19.
27. Perhaps the term "work" shares some of the possible ambiguities of "predict".
28. The mere truth of their implications would not be sufficient to make assumptions "simplifications of the truth" in the sense intended here. On the vagueness of "conventional", see page 4.15.
29. Some critics seem to have missed the point that not all the examples he canvasses are ones he recommends us actually to adopt.
30. Thus, rather as Hahn (1973, p.8) writes of Debreu, "odd though it is that so clear a writer...should be misread, it can be explained by a genuine problem".
31. See Chapter 3, note 29.
32. The diversions may not have been plotted ones: see page 5.13.

If they were, Joan Robinson's view of Marshall might come to mind: "The more I learn about economics the more I admire [his] intellect and the less I like his character...the thinner is the argument the thicker is the tear gas" (1973, pp.259 and 262).

33. Of course, a number do take some of Friedman's examples into account, as illustrations of his preaching. But they rarely consider them all; or discuss the differences between them; or criticise his translation of preaching into practice. Though examples may have triggered off

their chase, then, they think their prey more general.

34. On Friedman's standards, true assumptions are not necessarily the ideal either, unless they are simple as well. See note 14 and Appendix B.
35. See below, pp.4.15 to 4.16.
36. The divisions are designed to echo differences between Friedman's examples, and I think they will serve for that purpose.
37. Even a falsehood that broadly epitomised truth would be unsuccessful when used to predict (explain) the very facts that made it false.
38. These "as if" assumptions are those that the moderate case for "abstractive" falsehoods would justify.
39. He presents hypotheses based on assumptions-B as acceptable ones, but recognises that in fact preferable theories will often be available.
40. This may have been what Koopmans (1957, p.536) had in mind when he wrote that "in each of Professor Friedman's examples he knows more about the phenomenon in question than he lets on in his suggested postulates".
41. For some illustration of when assumptions-B might be "accepted"(in the sense of being adopted and made use of), see Appendix C.

(Friedman's use of the word "accept" may, incidentally, tend to mislead. For often we can "accept" in the sense above, when we cannot "accept", meaning "take to be true"

or "believe".) (See Klappholz and Agassi (1959)).

42. His common treatment of his diverse examples might seem to preclude him from distinguishing these two groups. But see note 39.
43. See note 28.
44. See Appendix B and Chapter 10, note 6.
45. Some of Friedman's examples make it tempting to say that the hypothetical motive forces introduced by assumptions-B will be impossible ones. But the distinction drawn does not require them to be.
46. Introduction of examples involving the controversial assumptions-B seems to have served to divert attention from this critical distinction.
47. Since conclusions drawn from assumptions A or B need only be very nearly true, the conclusions of all three groups of assumption can be false; and so the distinction/here must be one of degree.
48. A point to be exploited in Chapter 7.
49. See Chapter 10, note 5.
50. But she goes on to claim that they very often do. See Chapter 6, note 24.

Chapter 5

AN "AS IF" BUSINESS

The four key examples in Friedman's argument are presented in Section III of "The Methodology". First comes "a simple physical example, the law of falling bodies". Here Friedman suggests that "the application of [the] formula $[s = \frac{1}{2}gt^2]$ to a compact ball dropped from the roof of a building is equivalent to saying that a ball so dropped behaves as if it were falling in a vacuum" (p. 16). This example is, I think, the strongest Friedman gives in support of his idea of admitting false, but only "descriptively false", assumptions: we may treat what might conveniently be called this "assumption of a vacuum"¹ as a "white lie" - as an assumption-A.²

Next comes an example "designed to be an analogue of many hypotheses in the social sciences" (p. 19), an example concerning the density of leaves around a tree: Friedman suggests the hypothesis "that the leaves are positioned as if each leaf deliberately sought to maximise the amount of sunlight it receives, given the position of its neighbors, as if it knew the physical laws determining the amount of sunlight that would be received in various positions and could move rapidly or instantaneously from any one position to any other desired and unoccupied position". And "a largely parallel example involving human behaviour" follows. We are asked (p. 21) to "consider the problem of predicting the shots made by an expert billiard player. It seems not at all unreasonable that excellent predictions would be yielded by the hypothesis that the billiard player made his shots as if he knew the

complicated mathematical formulas that would give the optimum directions of travel, could estimate accurately by eye the angles, etc., describing the location of the balls, could make lightning calculations from the formulas, and could then make the balls travel in the direction indicated by the formulas". Both these examples involve false "assumptions" that do not, it seems to me, pass muster as assumptions-A. But since both hypotheses have true, or largely true, conclusions, perhaps the "lies" might qualify as grey, i.e. as assumptions-B.³

"It is only a short step from these examples", Friedman tells us (p. 21), "to the economic hypothesis that under a wide range of circumstances individual firms behave as if they were seeking rationally to maximise expected returns ... and had full knowledge of the data needed to succeed in this attempt; as if, that is, they knew the relevant cost and demand functions, calculated marginal cost and marginal revenue from all actions open to them, and pushed each line of action to the point at which the relevant marginal cost and marginal revenue were equal." But is the step a short one? This chapter will, I think, provide an answer.

The particular economic theory that I have argued Friedman seeks to protect and promote could, summarising his expression of it above, be expressed as the hypothesis that

- (1) all businessmen behave as if they were seeking rationally to maximise their expected returns and had full knowledge of the data necessary for success (i.e. as if they were "DRRMs" - deliberate rational returns-maximisers)⁴

Now he maintains (1) despite accepting, albeit sometimes reluctantly, evidence that

- (2) no businessmen do deliberately seek to maximise returns precisely; none does actually formulate and solve the returns-maximising equations, or have the knowledge necessary to do so.⁵

He maintains (1), then, even though presumably accepting that

- (3) no businessmen actually are DRRMs.⁶

(1) to (3) form an unusual and at first sight disarming set. Friedman's strategy in "The Methodology" appears to be to outflank critics of the maximisation-of-returns theory, by stating that evidence supporting (2) and so (3) is nonetheless consistent with the truth of (1). If this is so then, he suggests, bring what evidence the critics will in favour of (3), still they cannot dislodge his "as if" hypothesis thereby: all they will show is that its "as if" clause is not fulfilled, a fact he already admits.⁷ If we assume, for the sake of simplicity, something that is false, still some of what follows from our assumption may be true, and this is what Friedman relies on here. (1) claims that something follows about businessmen's behaviour from assuming them to be DRRMs and asserts the truth of what does; and what does follow must, for Friedman's case to hold, be true (or very nearly so).⁸

But can (1) indeed be maintained if (2) is true? Having accepted that all DRRMs proceed by solving the returns-maximising equations, how can Friedman agree that (2) is true, that no businessmen do solve these equations, without being committed to saying that no businessmen

behave as if they're DRRMs either? At best, (1) can only be true if "behave" is given a severely restricted sense.⁹ Now Friedman himself does seem to interpret (1) as referring just to a restricted zone of business behaviour: he apparently takes "behave" to mean "act in the market", seeing it as denoting the end products of business decisions and not the process of producing them. But if this is how (1) is to be interpreted, surely, for clarity, he should have made this plainer.¹⁰ Moreover, "behave" is not the only slippery, not to say treacherous, term in his hypothesis. For if (3) is true and the supposition of the "as if" clause in (1) can't be fulfilled, then the "as if" itself needs cautious interpretation. Since Friedman accepts (2), whence (3), he can't very well be using (1) to claim that, given the evidence of business actions in the market, businessmen may actually be DRRMs: he can't, that is, say "businessmen behave as if they're DRRMs" and then add, in good faith, "so maybe they are". Yet one might all too easily be beguiled by the "as if" into imagining by and by that businessmen's DRRM-ness is being established after all¹¹ - that somehow the truth of the whole hypothesis is mysteriously transmitted to its every part.¹²

The terms in (1) having thus been restricted, the issue now is, as what sort of assumption is the statement in the "as if" clause serving? Here Friedman's presentation is confusing. Sometimes he gives the impression that he takes the "assumption" of his hypothesis to be only technically (descriptively) false, remaining faithful to the broad essentials of the truth.¹³ For instance, whilst he accepts the evidence supporting (2), gleaned by asking businessmen questions about their

decision-making, at one point (p. 22) he also says businessmen do not "actually and literally" solve the relevant system of simultaneous equations; and he goes on to mention calculations not being gone through (for example, by the billiard players) "explicitly" (my emphasis both times^{14, 15}). This muted admission might suggest that he would claim it as an assumption-A, believing that little parts his economics hypothesis from the physical one about the behaviour of falling bodies.¹⁶ And indeed, he does later present "ideal types" in economic theory as if they are on a par with the ideal types of natural science, saying that they "are designed to isolate the features that are crucial for a particular problem" (p. 36), rather than being (p. 34) "strictly descriptive categories intended to correspond directly and fully to entities in the real world independently of the purpose for which the model is being used".¹⁷

As against this, however, when Friedman quotes, without demur, the judgement that "it would be utterly impractical under present conditions for the manager of a multi-process plant to attempt ... to work out and equate marginal costs and marginal revenues for each productive factor" (p. 32), he is surely giving tacit support to the view that the decision-making of businessmen working in complex modern firms¹⁸ is bound to be a far cry from that of the "ideal" DRRMs. What is more, immediately after writing that businessmen do not solve the equations "actually and literally" (where he does state too that "of course" they don't), he continues: "any more than leaves or billiard players explicitly go through complicated mathematical calculations or falling bodies decide to create a vacuum" - which must surely be to

say, by comparison at least with the leaves and these, now suddenly supposed sentient, falling bodies, that they do not do anything remotely like solving them.¹⁹ Here he is surely likening his DRRM assumption to assumptions that are not, after all, assumptions-A, and treating the leaf hypothesis as indeed "an analogue" of the economics one. Is, then, the "lie" about businessmen really any better than grey? What are the real "forces" that the DRRM assumption would be allowing us to consider in isolation, were it an assumption-A? Can there, for instance, be an all-pervading, non-fictitious force of conscious returns-maximising rationality, which is merely masked, distorted or disturbed by other forces (less "crucial" for business actions in the market) to become the apparently much weaker "profit motive" of observed behaviour? The very idea that human actions can usefully be thought of as the mere resultants of sets of forces, let alone of these particular forces, seems to me a dubious one.²⁰ But if the DRRM assumption were only an assumption-B, how important a role could it be playing in the theory of the firm? As an assumption-B, it would add no information about the actual behaviour of firms to that included in an assertion of its conclusions alone. - conclusions that would be true for reasons it gave no hint of.²¹ If the DRRM assumption is an assumption-B, then, Friedman exaggerates its value with any impression he leaves that it is but a "short step" away from his vacuum "assumption" too.²²

There is certainly something disturbing in Friedman's apparently indiscriminate use of expert billiard players, leaves, falling bodies and ordinary businessmen in his examples; and in the curious expressions

that seem to result from this and perhaps too from the slippery nature of "as if".²³ For instance, in the leaf example comes the very odd claim that "so far as we know, leaves do not 'deliberate' or consciously 'seek', have not been to school and learned the relevant laws of science ..." etc. (p. 20, my emphasis) - a strange juncture to choose for making any gestures towards a Popperian view of the conjectural nature of knowledge. Then again, there is the over-ready switch from the appealing hypothesis that bodies generally fall as if in a vacuum to the absurd-sounding one that they fall as if they first "decide to create" a vacuum and then fall in it.²⁴ Amidst these seeming confusions, one might very well begin to wonder how far Friedman does bear in mind the differences there are between his examples, and whether he is tending to treat the grey as white. But could we have been meant to wonder?²⁵ The very weirdness of his, very ingenious, examples that (in my terms) involve assumptions-B has of course provoked fierce discussion of whether having true implications makes a theory good enough, discussion long prolonged since so much seems to hinge on the questions "good for what? - and against what background?" And in the discussion, the presence of true (or nearly true) conclusions not being a point at issue, attention has been focussed on the significance of assumptions themselves instead. Once one is released from the distractions of Friedman's intriguing assumption-B examples, however, a new doubt about his economics hypothesis might arise. For is the DRRM assumption even an assumption-B? How certain is it that the DRRM hypothesis, (1), is true (or almost true)? If (2) is true, it is remarkable if businessmen can persistently pull off the maximising result; and one might well

wonder now what process there can be that actually is at work, as the counterpart of passive adaptation of leaf-growth to sunlight in that 'analogous' example, to bring about the outcome which is alleged.²⁶ Could it be that the DRRM assumption, deftly slipped in by Friedman amongst his already perplexing examples, is in fact an assumption-C? Does anything in "The Methodology" show that what follows about action in the market from assuming businessmen to be DRRMs is true (or very nearly so)?²⁷

Friedman first attempts to argue indirectly for the truth of the conclusion about business behaviour, an approach it is hard to square with the methodological principles he is so often held to profess.²⁸ He appeals to a parallel with his hypothesis about the billiard players and to the process of "natural selection". Confidence in the billiards hypothesis, claims Friedman earlier (p. 21), "is not based on the belief that billiard players, even expert ones, can or do go through the process described; it derives rather from the belief that, unless in some way or other they were capable of reaching essentially the same result, they would not in fact be expert billiard players". Now, he claims (p. 22) that similar "evidence" supports the maximisation-of-returns hypothesis: "unless the behavior of businessmen in some way or other approximated behavior consistent with the maximization of returns, it seems unlikely that they would remain in business for long". Consequently, "given natural selection, acceptance of the hypothesis can be based largely on the judgment that it summarises appropriately the conditions for survival".

Now appealing to the billiards hypothesis is a very subtle move; and

at first sight it might seem to be a powerful one. After all, the "as if" billiards hypothesis looks open to the objections that are commonly raised against the business one, but yet it seems broadly true: its implications about the upshot of the process by which expert players make their shots do seem to be ones that experience would, more or less, bear out.²⁹ And if the billiards expert is able to make his, often perfect, shots without consciously going through complex mathematical calculations, why shouldn't the businessman manage to maximise expected returns (or at least come close to maximising them), the questionnaire/evidence notwithstanding?³⁰ However, surely Friedman overplays the analogy here, setting standards for survival in business that, unlike those set for expertise in billiards, are ones that can't be realised. There can be confidence in the billiards hypothesis despite Friedman's vagueness about the method by which expert billiard players actually play so well (despite, that is, his telling us only that they must be capable of reaching superb results "by some means or other" (p. 21)) because it is easy to conceive what the nature of this method might be; and this makes the case of the expert billiard player crucially different from that of the businessman. Whereas the expert at billiards may be able to size up his shots with the aid of just one of his senses (sight), gaugeing the angles and distances by eye to within the margin of accuracy required by the game (and conceivably eventually even doing this without needing much recourse to conscious thought at all - passing beyond a phase of concentration to that of habitually fine play), the businessman by contrast has no comparable means of assessing a business situation

directly (no single sense seems suited for registering key factors; and conscious calculating thought is surely always bound to be involved, partly because allowance has somehow to be made for the dynamism of the system if maxima are to be achieved, partly because all the factors relevant to the business situation don't ordinarily fall within the businessman's immediate narrow field of sense perception at any one time - except perhaps in the form of ciphers, which by their very nature seem even more likely than factors directly present to call for conscious interpretation).³¹ And again, whereas the billiards expert has been able to gain his expertise at the game by practice, since he faces broadly similar situations again and again (level table, round ball, straight cue), by comparison the businessman, whether expert or only common-or-garden, has to respond to a state of flux in which comparatively little need be remaining constant, and so would have very much less chance of achieving the maximising outcome as a result of the process of "learning by doing".³²

Here Friedman would protest that speculation about what decision-making processes are available to businessmen misses the point he is making. For, he would say, it is the play of impersonal market forces that sets the standard for survival in business so high³³; and we can know that this standard must be being reached in businesses that survive, however much in the dark we may be about how it is. "Let the apparent immediate determinant of business behavior be anything at all", he writes, "habitual reaction, random chance, or whatnot. Whenever this determinant happens to lead to behavior consistent with rational and

informed maximization of returns, the business will prosper and acquire resources with which to expand; whenever it does not, the business will tend to lose resources and can be kept in existence only by the addition of resources from outside" (p. 22)³⁴. Now since many businesses do survive, it isn't possible to maintain both Friedman's argument (that (1) summarises the conditions for survival, the process of natural selection dooming any deviant decision-makers to extinction) and the argument I am suggesting (that no real-world businessman could attain the maximising standard being laid down).³⁵ But is Friedman's argument correct? Are the conditions of market behaviour summarised in (1) really required for survival? Surely not. A comparison between the conditions presented as necessary for business survival, in "The Methodology" on the one hand and in Alchian's 1950 article on evolution in economics (an article which Friedman claims sympathy with) on the other, reveals striking differences; and Alchian's natural selection argument is surely much the more successful. He was seeking an alternative approach to market behaviour to that relying on the insubstantial notion of maximum profits, a notion he held to be meaningless in conditions of uncertainty; and he argued that, in general, surviving firms must make positive profits over a period, stressing that only in the special (and hypothetical) conditions of perfect competition would it be true that maximum profits are required for survival.³⁶ Friedman distorts this argument. Surely achieving the maximum (or even a near maximum) of expected returns is not the sine qua non of persistence in business; and surely Friedman's business hypothesis "summarizes ... the conditions for survival" inappropriately.³⁷ The

perfect competition of the jungle may permit only the fittest to survive; but the human institutions of the market seem able to countenance at least all the fit.³⁸

Still, if Friedman's business hypothesis has successfully been put to direct predictive test, mustn't my objections to the indirect backing he tries to give it have been at any rate over-strong? In outlining his direct evidence, Friedman begins bravely: "an even more important body of evidence for the maximization-of-returns hypothesis", he writes, "is experience from countless applications of the hypothesis to specific problems and the repeated failure of its implications to be contradicted" (p. 22). But what follows appears uncomfortably like evasion. He cites specifically none of the instances in which the hypothesis has (on his claim) been satisfactorily applied; and remarks instead that "this evidence is extremely hard to document ... the evidence for a hypothesis always consists of its repeated failure to be contradicted ... and by its very nature is difficult to document at all comprehensively. It tends to become part of the tradition and folklore of a science revealed in the tenacity with which hypotheses are held rather than in any textbook list of instances in which the hypothesis has failed to be contradicted." Now whilst Friedman might perhaps legitimately appeal to the difficulty, with a universal statement, of ever completing documentation of favourable instances, this is surely quite different from his apparent difficulty in even beginning it. In this context, when he himself has set such store on the failure of others to offer direct evidence (see, for instance, Friedman, pp. 15 and 31) and when his own indirect arguments seem

to be ones that fail, these remarks must surely appear as an admission of weakness.³⁹ It is hard to avoid suspecting that behind Friedman's omission of specific citation of any direct evidence here may lie the fact that there simply isn't any. (Indeed, in this case one wonders whether there could be any. Friedman seems to admit that businessmen do not explicitly calculate the relevant maxima because, amongst other reasons, it would be impossible for them to do so. But then why should the watching economist be in any better position to measure fleeting marginal quantities and to gain direct evidence that maxima have actually been achieved?)⁴⁰

Yet Friedman himself seems to take his "as if" hypothesis about business behaviour to be true, or very nearly so. I fancy that he may do so as a question of faith, rather than of observation, though. It is clear that he thinks of the returns-maximising outcome as virtually inevitable, with natural laws operating unfailingly, if mysteriously, to bring it about, whatever businessmen may say or feel. And ^{it} seems to be a matter of belief with him that particular states of conscious knowledge have no more relevance to business actions than they do to the behaviour of leaves or inanimate falling bodies. He seems to believe that, just as natural laws may fill up the gap left by those fictitiously false assumptions that demand consciousness on the part of leaves or falling stones, so, in the absence of living DRRMs, we must look to natural laws rather than to any different form of deliberate human action as the determinant of business behaviour. This fatalistic attitude to the businessman's destiny is evident again in the quip (p. 31) that testing

the maximisation-of-returns theory by "the answers given by businessmen to questions about factors affecting their decisions [is] a procedure for testing economic theories that is about on a par with testing theories of longevity by asking octogenarians how they account for their long life."

Here another analogy is overplayed: we may reasonably take businessmen's achievements as being at least in part the result of deliberative decisions which they could surely sometimes in fair measure account for; whereas there is generally reason to doubt whether octogenarians can have had much conscious influence over the age that they have reached, and so we lack the special grounds which we have in the sphere of business action for asking them to explain how it came about.⁴¹ It does not follow from there being natural laws in the purely physical world that there will be an "invisible hand" in the business one, guiding men's affairs irresistibly to a pre-ordained destination and making futile any human attempt to steer a different course.⁴²

Without Friedman's faith in some such irresistible power of the market-place to serve as a deus ex machina for his "as if" theory of the firm, is this a theory we can reasonably accept? Could his DRRM assumption lead even the vicarious life of an assumption-B? I do not think so.⁴³ For picture the businessman beloved of the questionnaire accounts. He professes an aim quite other than that of maximising returns.⁴⁴ He fails to answer correctly questions designed to test his grasp of the significance of the marginal measures, and his knowledge of their magnitude in particular cases. He cannot be detected going through the appropriate problem-solving, information-seeking motions;

and denies doing so. Now we are asked to believe that, not just once but time after time, he nonetheless charges the price that maximises his expected returns, when it comes to the point. Unless the man is systematically setting out to deceive us, isn't this implausible? Can his true aim always be frustrated, and his deficient knowledge always be made good, by just such a bungling in his execution of his plans as will exactly compensate for their originally divergent direction? Can he be a returns-maximiser quite in spite of himself? Surely the story is far-fetched.

I suggest, then, that the constellation of truth values that Friedman so ingeniously argues for in the rest of his "Methodology" (false assumption: true, or very nearly true, conclusion) doesn't and couldn't exist in his central economics case. Then any followers of Friedman who happily swallow the story that businessmen do behave in the market more or less as the DRRM assumption would imply might indeed, vaguely conscious of some necessary linkage here, feel this "as if" assumption must have turned up trumps after all and somehow shown itself in all essentials true.⁴⁵ Then too, small wonder that critics have felt that all was not well with "The Methodology".

If I am right, then, for the purpose of vindicating his "as if" business theory, all Friedman's argument (on whichever interpretation) proves in the end to be beside the point - for all its effect against naive criticisms of theory of the firm and its success as a diversionary tactic. Using his "grey lies" both as a decoy and as a seeming bridge, he has put up a spirited defence of "white lies", only to tell one that is black.

And whilst opponents of "The Methodology" (justly sensing it to be somewhere at fault) have futilely tried to insist on white lies being treated as black, Friedman's supporters (with equal justice, feeling him often to be in the right) have sought, with no greater success, to pass off the black lie as white.

Thus "The Methodology", having created an impression of paradox, has itself been fundamental in the Mystery of its worth. And while the Mystery has deepened, with the vital clues so well disguised in Friedman's broader theses, the maximisation-of-returns hypothesis has been able to persist, perhaps as the prime example of a theory that, though "unsuccessful", yet proves "slow and difficult" to weed out: the theory among theories of those that, in Friedman's (p. 11) words, "are seldom downed for good and are always cropping up again".

Appendix VFootnotes to Chapter 5

1. A phrase that Friedman does not welcome (p.18) but that is surely harmless here.
2. That is, as an assumption A in a wide range of particular uses.
3. On the background mechanism which assures success for the billiards hypothesis, see pages 5.9 to 5.10.
4. Friedman speaks of firms and businessmen interchangeably and seems to intend his claim about behaviour to apply to all firms.
5. Again, his acceptance that "of course, businessmen do not actually" do these things (p.22) seems to be intended as a claim about all businessmen. On his acceptance of the evidence, see pages 5.4 and 5.5 (and notes).
6. On how far the "actually" here may be significant, see below.
7. See Appendix C.
8. If we take it that what does follow about business behaviour (in any sense of "behave"(see below)) from assuming businessmen to be DRRMs is not a question of dispute, then (1) stands or falls on the truth of its conclusion. On the use that Friedman might make of (1), given his acceptance of (3), see parallels in Appendix C.
9. The leaf and billiard player analogies give the clue to what the sense Friedman intends might be. Neither the leaves

nor the players calculate; but for the former "their density is the same as if they did"(p.20), whilst the latter are "capable of reaching essentially the same result" as the calculated one. This suggests that Friedman may take "behave" in (1) to mean "act in the event", rather than "behave in all respects".

10. With no restriction on the meaning of "behave" made explicit, (1) might very well be understood as the hypothesis that businessmen behave in all respects as if they're DRRMs. But, even for the non-behaviourist, it is hard to imagine businessmen behaving in every respect as DRRMs without also actually being DRRMs too. If Friedman's claim that (1) has survived attempted refutations is accepted, one may then be misled into supposing it ^{been} must somehow have/shown that the questionnaire evidence was false.
11. See Appendix C.
12. It is suggested below that the transmission mechanism here is not itself imaginary but that its being in operation is.
13. The essentials, provided literal description of businessmen's decision procedures is not our question of concern (see Chapter 4, note 37).
14. On the relative scope of billiard players and businessmen for making "implicit" calculations, see pages 5.9 and 5.10.
15. On the scope for leaves calculating "implicitly", see below.
16. Rosenberg (1972, p.27) claims that Friedman does not admit

the questionnaire evidence as supporting (2) and (3) at all, and that indeed he excludes the possibility of such evidence being relevant to "microeconomic general statements". But I think this claim rests on a misunderstanding of what Friedman means by "the associated hypothesis" in the (p.22) passage of "The Methodology" that Rosenberg cites. (See also Appendix A).

17. See page 4.12, and Friedman's (p.40) passage quoted there.
18. Perhaps he would not extend the view to cover every modern firm, however.
19. On the billiard players' case, see pages 5.8 to 5.10.
20. Little agrees (1950, introduction): "In psychology...the physical analogy has proved barren. It is not useful to think of the mind as consisting of molecules - feelings and volitions - tugging this way and that, with a resultant force which realises itself in action."
21. But could these conclusions be true for independent reasons? See below.
22. Doesn't the step seem a long one, between neglecting air pressure so as to concentrate on gravity, and neglecting irrational behaviour so as to concentrate on rationality? See note 20 and note 33.
23. See Appendix C.
24. The new assumption stated is surely not an assumption-A, for all the fact that the "as if" clause still makes mention of a vacuum.

25. I think that such a policy of making us wonder might perhaps be a subconscious one (see page 5.13).
26. Friedman himself introduces the hypothesis of passive adaptation in the case of the leaves, suggesting it to be a preferable hypothesis (see Chapter 4, note 39). As Koopmans has pointed out, if we knew there to be a comparable process backing up Friedman's business hypothesis, there would be a preferable business hypothesis too.
27. If what follows is neither true, nor nearly true, the assumption is an assumption-C (see pages 4.15 and 4.16).
28. This indirect argument for the conclusion's truth seems consistent with the moderate methodology described in Chapter 4, but would suggest, I think, use of an assumption other than the DRRM one. See note 26.
29. In the sense that they will typically make shots that are "perfect", within the margin of accuracy set by the game. The pockets being wider than the balls are broad and getting a ball into a pocket often being the best shot, the ball that is struck need not travel precisely along a single central line for the ideal upshot to result. There will be, as it were, a "beam" of equally good possible paths to the broad goal. (Does Friedman take the "optimal direction" to be a line, just as the perfectly competitive "path" has to be strictly narrow? See note 30, and page 5.11).
30. With some of Friedman's non-economic hypotheses, coming

close to the precise result implied can seem a realistic possibility although achieving it precisely is not. But for his economics case there prove to be special difficulties in connection with approximating the perfect result. See note 45; and Chapter 7, especially page 7.9.

31. The suggestion, then, that businessmen somehow simply "sense" or "feel" what each new set of maximising requirements is seems to be a non-starter. See note 32, too.
32. To put it another way: if, at each game, the billiard table had a different slant and its surface a different configuration of hollows and humps, if the balls were differently shaped and the cues differently bent, he who, with the aid just of his "practised eye", always managed to score would be not merely an expert player but also an immortal one.

Not that the businessman lives in a realm of total chaos. Though too little will remain constant for the development by practice of "fine tuning" skills, still enough may be remaining in a similar zone to give some broadly stable base for decision-making. But a policy of making discrete changes (taking fewer, larger steps) might now be "second best" (Achilles stepping over the tortoise? Morgenstern (1973, p.1171) has similarly suggested relating the tatonnement process to Zeno's paradox.) See note/41; and Chapter 7.

33. Couldn't it be these impersonal forces of the market that the DRRM assumption allows us to consider in isolation? (See above and note 22). Some abstract assumptions might approximately fulfil this role, I think (see notes 26 and

42), but not the DRRM one; for the impersonal force it might be held to isolate is not one of actual markets. See page 3.11 on Alchian.

34. See note 4.

35. What counts as "behaviour consistent with rational and informed maximization of returns" (my emphasis)? As I have taken it in stating Friedman's argument, the market behaviour that DRRMing does imply (but would not in fact be responsible for); and Friedman's argument can then be attacked on the grounds that such behaviour is not, as he claims, necessary for survival. But the phrase may be another slippery one; for it might instead suggest behaviour that some would think of as tending in the same direction as DRRM behaviour (e.g. pursuit of positive returns, rather than maximum ones). If the latter were what Friedman meant, his view of the survival process might not be wrong after all - but his hypothesis (1) could no longer be presented as summarising the survival conditions. See below, on Alchian.

36. For the perfectly competitive situation is what Latsis (1972) calls a "straightjacket" (or "single exit") one: "the 'nature' of perfect competition is unusually strict in allowing a choice of either following a single strategy or going under" (p.210). (However, in these circumstances, as Latsis has pointed out, it cannot really be said that decision-makers freely choose to maximise profits, in preference to making, for instance, merely positive ones: "to say that a seller under perfect competition chooses a

course of action to maximise profits is analogous to saying that a member of the audience is maximising if he runs out of the single exit available to him in a burning cinema." Thus the "maximising" policy is also a "bankruptcy avoidance"one.)

But out of the perfectly competitive situation, on the Alchian argument, "Realised positive profits, not maximum profits, are the mark of success and viability. ...This is the criterion by which the economic system selects survivors: those who realise positive profits are the survivors; those who suffer losses disappear."

Alchian's argument seems to have been widely misrepresented, and confused with the, surely false, claim that only maximisers survive. Not only Friedman but also Penrose (1952 and 1953) in her hostile replies to Alchian's paper, give his case this gloss. Yet Alchian himself particularly emphasises that "the pertinent requirement - positive profits through relative efficiency - is weaker than "maximised profits", with which, unfortunately, it has been confused."(p.213) He writes that "positive profits accrue to those who are better than their actual competitors, even if the participants are ignorant, intelligent, skilful, etc. The crucial element is one's aggregate position relative to actual competitors, not some hypothetically perfect competitors. As in a race, the award goes to the relatively fastest, even if all the competitors loaf. Even in a world of stupid men there would still be profits. Also, the greater the uncertainties of the world, the greater is the possibility that

profits would go to venturesome and lucky rather than to logical, careful, fact-gathering individuals."

It is because of Alchian's paper that Friedman avoids the term "maximum profits" and speaks instead of "expected returns", as a footnote in "The Methodology" explains (p.21, n.16). But Friedman ends his note rather lightly with the comment that "the issues alluded to ... are not basic to the methodological issues being discussed, and so are largely by-passed in the discussion that follows", going on to maintain the idea of maximising these returns. It may be that the issues aren't fundamental to broader questions of scientific method (though see note 42), but it is surely false that they are not fundamental for the application of Friedman's methodological precepts to this particular economics case, discussion of which is what follows. (See note 40).

37. Perhaps he himself senses that he has done so; for, in saying merely that "it seems unlikely" that businessmen who fail to approximate conditions summarised in (1) would survive, and in recognising the possibility of, as it were artificially, prolonging existence by "the addition of resources from outside", he already weakens any claim of his hypothesis to be fulfilled.
38. And the mixed economy even accommodates "lame ducks".
39. But not an explicit one. Yet what argument could Friedman now bring to bear if the theorists of imperfect competition felt tenacious too?
40. Does the fact that Friedman treats of "expected returns"

rather than profits mean the relevant maxima are observables? And, more generally, could Friedman seek to salvage his hypothesis with the claim that due attention hasn't been paid to his concern only with expected returns (interpreted by Friedman as expected receipts and/or the expected utilities associated with them (p.21, footnote 16))? Perhaps if the hypothesis is meant merely as the claim that businessmen do the best they can to get such returns as they want, it might be true. But if it were, it would be trivial, and would seem to be consistent with the theories Friedman rejects. (See note 36).

41. Rosenberg (1972, p.28) criticises this analogy in similar terms, as does Coddington.

Is the difference between the cases a matter of degree? Perhaps so. On the one hand, the octogenarian may at least have deliberately avoided activities which would (certainly?) have shortened his life (e.g. smoking). And though the businessman, on the other, may give an account of acts that contributed to a final achievement, his influence on events will be incomplete (in his environment of uncertainty and change). (See note 32).

42. Where the destination is defined as a highly specific one. However, the broader Alchian evolutionary approach may have something to offer.

43. Can it be said that truth of the DRRM assumption is necessary for achievement of the maximising result? Surely, businessmen would reach the same result if they actually sought to minimise returns but persistently

made an arithmetical error (I am grateful to Kit Fine for this point). And this suggests a variety of other sufficient assumptions, albeit impossible ones, might be imagined here too (see Chapter 8, notes 10 and 12). However, the DRRM assumption would be an assumption-C, even if only sufficient for its conclusion, if no other sufficient assumption were in fact true - and surely none is. (On the possible "survival" assumption required to yield the maximising result, see note 36.) The DRRM assumption is necessary, then, in the perhaps rather special sense that, given the fact, that all other sufficient assumptions are false, only if it were true could its conclusion be too.

44. I take it that, except in the perfectly competitive situation, an aim of satisficing, for instance, is quite distinct from a maximising one. See note 36, but also note 40.
45. See notes to page 5.4, and note 43.

Chapter 6TAKES STOCK AND LOOKS AHEAD TO A DILEMMA

"When she was good, she was very, very good;
But when she was bad, she was horrid."

Traditional nursery rhyme

The last two chapters have presented what I believe to be the solution to the Methodology Mystery, the answer to the question why consensus on the value of Friedman's case has been so especially slow to develop. That so many commentators have in various ways misrepresented his case forms part of the solution but turns out to be even more significant as a clue pointing towards the rest. For finding that much of what Friedman explicitly says about method¹ proves respectably run-of-the-mill leads one to ask what can have caused hosts of critics to think it instead either the product of only a narrow school of thought that they see as plainly inferior; or else more radically uninformed, and revolutionary but outrageously wrong. Would most logicians agree with Friedman that the falsity of a theory's assumptions doesn't necessarily carry with it the falsity of its conclusions? - Surely the answer is, yes.² Do most philosophers of science support Friedman's view that theories can often be used to advantage although there is known to be evidence that tells against them? - Yes again.³ But critics of "The Methodology", perhaps misled to some extent by a certain grandiloquence not altogether suited to an essay of limited scope, and by views that are incautiously worded when they run the

risk of being read in isolation, do not take these points to be ones in Friedman's favour; for they see all his claims not only as much stronger than these, but also as strong enough to prohibit him from holding a moderate view on any other matters of method. Many might here insist that Friedman is outrageously wrong in saying that a false assumption is always better than a true one - as indeed he would be, if he did say this. But it is by no means clear that he does: what he says is at the very least compatible with recognition that true assumptions may have value as well as limitations, and false ones limitations as well as value. Arguing for "telling fibs" only in restricted situations, Friedman would surely want to claim "whiteness" for his lies. That, might return the critics, is just an attempt to whitewash; and an attempt that furthermore won't wash, for what, after all, does this "whiteness" amount to? - surely, for Friedman, just to the yielding of valid predictions; and then isn't he as wrong as ever, still tolerating grossly false assumptions and now showing himself as obsessed with the goal of prediction to boot? But "prediction" can reasonably be used in a sense wider than that in which the critics have generally understood it;⁴ and Friedman's argument, it seems to me, is driving at the idea that "whiteness" can be claimed for some false assumptions on the grounds, not solely that their implications happen to be true (or very nearly so) but also that they epitomise truth in some more direct way.⁵ He deftly suggests that attempts to epitomise truth are necessary and that they can result in statements that themselves fail of literal truth; and this is the mainstay of his particular point that truth should not always be viewed as a strict

requirement for statements that are to serve as assumptions.⁶ Yet it is a mainstay that is often overlooked; and here the oversight may be fostered, I think, not just by Friedman's manner of phrasing his case in "The Methodology", but also by the critical fact that not all the types of false assumption he introduces in his examples can be said to epitomise truth in the way his argument would seem to require.

It is at this point that Friedman's opponents seem originally to have gone astray; for they appear to have taken it for granted that Friedman's examples and his arguments would be in step. Then, rightly believing that an error was there to be detected, they have fruitlessly sought it, and have rashly proclaimed it, in his preaching on method, instead of scrutinising and exposing his translation of this into practice.⁷ Yet how far Friedman's argument will, even if valid, give backing for the examples he then proceeds to introduce is an aspect of "The Methodology" that the critics should perhaps have been alert to from the first; for Friedman's chief aim in his essay is surely to buttress just one assumption: the essay is surely designed to uphold a particular theory of the firm. It is in the pursuit of this aim that I think Friedman's error comes; for, pursuing it, he trades too far on the facts, firstly, that one false assumption he discusses (that of the theory he borrows from physics) enjoys a special relationship with the truth,⁸ and secondly, that in all his non-economic hypotheses, truth of their assumptions happens to be unnecessary for the truth (or at least, the near-truth) of their conclusions - taking a licence that enables him to slip his central economics example seemingly smoothly into the flow of argument in

"The Methodology", even though the truth-values in this example, I suggest, don't accord with the pattern (^{simplifying} assumption that is false: conclusion that is true; or, if not true, very nearly so) that Friedman has so ingeniously been championing. This seems to me to be the key to the solution of the Methodology Mystery. For, if I am right, Friedman's telling of the black lie is so subtle and his integration of it into the able defence of white lies so apparently complete, that its peculiar colour has then simply not been suspected by either side in the dispute it has confused.⁹

Thus, solving the Mystery proved to be a much longer task than, at the outset, I had expected it to be - but also, a far more exciting one. The initially queer-sounding claim that, in judging a theory's assumptions, one may not always be wise to insist on their honesty, is one that turns out to be too reasonable to be summarily dismissed.¹⁰ Yet, for all that, it fails to prop up the tottering economic theory that Friedman tries to champion, whose success does seem to depend on its assumptions "telling nothing but the truth". Both these findings, it will be argued below, have importance for the issue which is the subject of the following chapters - a central issue (some would say, the central issue) in economic theory.

The issue is, that of the value of general equilibrium (GE) theory (and of the derivative "neoclassical" - sometimes called "neo-neoclassical"¹¹ - theories, Friedman's hypothesis about business behaviour among them)¹²: a theory whose assumptions are widely acknowledged to be far removed from many aspects of reality; but one

that, nonetheless, still forms the basis of most undergraduate textbooks, is at the core of many degree courses in economics, and is sometimes proffered as a frame of reference for assessing government economic policy. Speculation about the future of the discipline still so much dominated by this theory¹³ is, however, rife and is fast establishing itself as a favourite theme - generally a gloomy one - for Presidential Addresses to Associations of economists (establishing itself in what Nelson and Winter describe as "a remarkable surge of authoritative grumbling" (1974, p. 890)¹⁴). By 1971, Leontief was quoting the words of a recent president of the Econometric Society: "the achievements of economic theory in the last two decades are both impressive and in many ways beautiful. But it cannot be denied that there is something scandalous in the spectacle of so many people refining the analysis of economic states which they give no reason to suppose will ever, or have ever, come about ... It is an unsatisfactory and slightly dishonest state of affairs". Then Leontief added himself: "But shouldn't this harsh judgment be suspended in the face of the impressive volume of econometric work? The answer is decidedly no" (1971, p. 2). He was followed in 1972 by Phelps-Brown, who noted (1972, p. 1) "the smallness of the contribution that the most conspicuous developments of economics in the last quarter of a century have made to the solution of the most pressing problems of the times"; himself hotly pursued by Worswick,¹⁵ whose contention (1972, p. 74) was that "one cannot avoid some uneasiness. The standards are high, the intellectual battalions are powerful, but ... the performance of economics seems curiously disappointing". MacDougall, feeling in 1974 that the time had come

"to redress the balance", starts his Address with the ingenious point that "self-criticism ... is by no means an unhealthy development"; but even he goes on to say: "I happen to agree with many of the criticisms that have been made" (1974, p. 773).¹⁶

In this doom-laden atmosphere,¹⁷ many economists are now willing to acknowledge that GE theory, in its traditional form,¹⁸ does not offer in itself a comprehensive explanation of the economic world. Some, however, are still reluctant to jettison the theory altogether, cherishing the hope that it can be reformed and so defended.¹⁹ At the same time, a considerable, probably growing group of economists clearly deem GE theory more deeply unsatisfactory than this, holding that its deficiencies are far past repair and that it should be replaced by a radically different theory.²⁰ Towards the end of his book, "Anti-Equilibrium", Kornai distinguishes two currents of this kind in the recent response to GE theory, calling the one "reformist" and the other "revolutionary". But the prospect he sees for reform is bleak: "each of the reformers", he writes, "makes a small dent in the foundations of the GE model [hoping] that the impact of his attack will destroy a dilapidated wing of the building, but [supposing] that the other parts will remain intact. However, if all attacks on the foundation were made simultaneously, the entire building would collapse." A similarly sceptical view of what the reformers are achieving is held by Kaldor²¹: taking up the metaphor, he writes (1972, p. 1239) that "the process of removing the 'scaffolding', as the saying goes, - in other words of relaxing the unreal basic assumptions - has not yet started. Indeed,

the scaffolding gets thicker and more impenetrable with every successive reformulation of the theory, with growing uncertainty as to whether there is a solid building underneath." And the total collapse of such a building as there has been supposed to be is the subject of such eager expectation with many revolutionaries that, unwilling merely to wait for reforming zeal to prove self-defeating by undermining the central structure with its all-round alterations, and impatient of the building showing itself to be chimerical or, if real, yet crumbling of its own accord, they are urging direct action to bring it down: Kaldor seems to be among them, for (1972, p. 1240) he has advocated "a major act of demolition" of the conceptual framework of the theory, holding that without it "it is impossible to make any real progress".

Yet the collapse, whether accidentally developed from within or deliberately engineered from without, has not yet come²²; and reformists²³ remain unconvinced that it need come ever. Because I believe that the theory does collapse, I would like to try to convince them. At the same time, I agree with Hahn that Ellman's view - that the teaching of GE theory is "a pernicious practice which does substantial harm" and that so far as possible it should be "remove[d] from the syllabus" (1972, p. 1481) - is "unattractively illiberal" (Hahn, 1973, p. 5)²⁴. The liberal Mill would certainly have thought so too; for he wrote that those who "refuse a hearing to an opinion because they are sure that it is false ... assume that their certainty is the same thing as absolute certainty. All silencing of discussion is an assumption of infallibility."²⁵ If GE theory does collapse, then it seems to me, on

the one hand, that it would not be pernicious to teach what the theory is and what its weaknesses are, and on the other, that reformists themselves, once convinced, might often be in favour of the present stranglehold the theory has in some quarters being relaxed, to allow fresh theories in. Would present defenders of GE theory ever be convinced that the theory must collapse? Some would argue that most would not: that their vested interest in both the theory and the value-system it is associated with would prove to be too strong.²⁶ But even if only a few were persuaded, should not revolutionaries think this worthwhile? - if not because they agree that, if revolution is to be, far better that it be peacefully brought about, then because the ruin of the theory they look forward to will surely be seen much more certainly and swiftly (and will be cleared up a great deal more tidily too) if but a handful of erstwhile reformers could be enlisted to work on the side of more radical change.²⁷ In any case, I am more hopeful that defenders could be convinced²⁸ - if, that is, the arguments brought against GE theory really are "convincing" ones.²⁹

Now I think that, if the reformists are to be convinced, two (from this point of view, complementary) lines of argument must be shown to have force against GE theory; and so my attempt at convincing falls into two parts. The first (given in Chapter 7, and building on the discovery that the falsity of Friedman's assumption does matter for his business hypothesis) argues that damage to the whole structure of this particular theory follows automatically on the slightest damage to any part of its foundations (damage there is acknowledged to be); whilst

the second (described in Chapter 8, and asking what limits can be set on how far the process of abstraction may legitimately go, given Friedman's point that invoking the criterion of literally "telling the truth" may not do) attempts to show that the theory is unsatisfactory even if presented (as it recently has been) as merely an abstract design whose value does not depend on its ever being implemented, whether perfectly or not.

Together, these two lines of argument do persuade me that GE theory does indeed collapse. At the same time, it must be admitted that, both for the details of the theory itself and for some points in the arguments against it, I have had to draw on what might be characterised as only hearsay evidence (for example, verbal statements of mathematical proofs, where the proofs seem to have been accepted in the literature, have simply been accepted on trust)³⁰. This introduces an element of doubt; but one that I have tried to keep within bounds by going for evidence, wherever possible, to the practitioners of GE theory themselves: a key argument against the theory in Chapter 7, for example, rests on a mathematical proof worked out jointly by the author of a standard British textbook on traditional neoclassical economics and by an economist well-known for developing theorems in, for instance, the neoclassical theory of international trade.³¹ This strategy of seeking information from the "adversaries"³² is one by which I hope to have minimised the risk of attributing to GE theory, and faulting it on, features that its defenders deny it has - a danger two major economists have recently exposed themselves to, on the testimony of a third: Hahn judges that

Joan Robinson's view of the neoclassical tradition "is very much like that of a medieval citizen of Lincoln of the Jews. Like him she attributes quite absurd beliefs and takes it for granted that these reflect wickedness" (1972, p. 205); and it is Hahn too who holds that, in attacking equilibrium theory, Kaldor has been "tilting at the windmill of some old fashioned textbook" (1973, p. 32). With some thought for the safety of my skin, a good deal of my hearsay evidence has been drawn from Hahn himself.

But because the evidence is in a sense hearsay, and sometimes circumstantial too, it could be that it misleads. Yet if it does, the arguments are still, I think, important ones. For it seems to me that if superior evidence were indeed to show that these arguments do not after all bear jointly against GE theory, then this theory may well be one that cannot be faulted categorically.³³ And I think that, as Chapter 9 will suggest, many attacks on the theory, along with attempts to promote rival ones, can be seen to have been weakened by their failure to take both lines of argument into account.

The arguments of the succeeding chapters, then, will, I suggest, break through both the possible lines of defence of GE theory. And its defenders will be placed on the prima facie horns of a dilemma. For, if I am right, any defence of the theory will only be able to escape Chapter 7's argument against it at the price of falling a victim to Chapter 8's; and vice versa.

This dilemma facing the would-be defenders might perhaps be more colourfully put in the terms of a traditional nursery rhyme. For

Chapter 7 will suggest that when the theory is bad, it is "horrid". And the burden of Chapter 8 is to be that if the theory were good, it would be a deal too good - too good to be true, and so good that it is, literally, quite out of this world.

Appendix VIFootnotes to Chapter 6

1. For in "The Methodology", he may not be offering a full account of scientific method. See page 4.2.
2. For objections brought against Friedman on points of logic, see Appendix A.
3. See Chapter 3, especially note 19 and page 3.10.
4. See page 3.10.
5. See pages 4.11 to 4.15.
6. Should not, at least in our present state of knowledge. See Chapter 4, note 14. See also Appendix B.
7. See Chapter 4, notes 7 and 31.
8. In its standard use. See the end of Chapter 4, and Chapter 5.
9. The appearance of integration is aided by Friedman's acceptance of "grey" lies in practice too. Friedman may well believe that the economic lie is better than black: see page 5.13.
10. Are there "instrumentalist" overtones in speaking of the "best policy"? Are false assumptions only to serve as a convenience; or may they be a necessity? (See Appendix B; and Chapter 4). Does the policy aid the eventual achievement of theories that can be "accepted", in the sense of being believed? When Friedman is given the benefit of any doubt on these scores, his claim has at least prima facie plausibility; but even when such benefit is allowed, his claims

for his central business hypothesis fail. See Chapter 4, especially page 4.10.

11. Neo-neoclassical is the term Joan Robinson uses, followed by some of the Cambridge school, for post-Keynesian economics in the traditional equilibrium mould. Neoclassical then refers, for them, to Marshall and Pigou et al, leaving the term classical for the group that includes Ricardo and Marx. But I follow perhaps the more usual usage in distinguishing only two groups.
12. See Chapter 7. Friedman's articulation of this hypothesis is novel; but see Chapter 5. On the dangers of making precise claims about details of the central theory itself, see below, and Chapter 7, note 7.
13. Those who have recently been critical of the present state of the discipline as a whole have clearly had traditional equilibrium theory very much in mind.
14. Nelson and Winter add an Address by Hahn to their list.
15. Worswick confirms the congruence of these assessments, saying (1972, p.73) that while preparing his own lecture he was "twice overtaken by presidential conveyances [Leontief's and Phelps-Brown's] moving very fast in the general same/direction".
16. MacDougall's move, in acknowledging the criticisms but claiming they are a sign of a "flourishing subject that can take such self-criticism in its stride" is reminiscent of Hahn's (1973, p.12) who, having acknowledged a serious difficulty for Arrow-Debreu equilibrium theory

goes on to claim: "it is one of the great virtues of the way good economic theorising proceeds that it allows us to pinpoint difficulties precisely and to be precise about the difficulties". Both their claims may be true and may give the impression that the account has been squared; but of course neither in itself can provide a rebuttal of the criticisms made. (For more on the difficulty prompting Hahn's remark, and his subsequent attempt to meet it, see Chapters 7 and 8). See also p.5.12.

17. In 1971, Leontief wrote: "The feeling of dissatisfaction with the present state of our discipline which prompts me to speak out so bluntly seems, alas, to be shared by relatively few"(p.3). But a number of others have spoken out since then.
18. On the form of equilibrium theory recently suggested by Hahn, see Chapter 7, note 8.
19. There may be some who will cherish the theory, come what may. But see Chapter 9.
20. Do they favour a theory whose fundamental assumptions are different, or a theory of a radically different kind - or might these possibilities reduce to one? See Chapter 9.

The proliferation of attempted modifications of the theory on the one hand and the growing dissatisfaction with it on the other have led a number of commentators to suggest traditional theory has reached a point of crisis. Nelson and Winter say (p.890) that "perhaps /a/ crisis is building now"; whilst several recent commentators have presented the situation in Kuhnian terms. And the Lakatosian concept

of a "protective belt" might well be applied.

21. Challenged by Hahn: see Chapter 7 (especially page 7.6 and note 16).
22. Kaldor's proffered demolition job would have been stronger, I think, had he taken into account Friedman's case on "abstractive" falsehood (see Chapter 2 and Appendix B). Joan Robinson's criticisms of the theory have gone a long way towards undermining it, I think, but are sometimes so epigrammatically expressed that it is hard to be sure. See Chapter 9.
23. In adopting the labels "reformist" and "revolutionary" in much of what follows, I may be inviting the charge that I too indulge in "facile pigeon-holing" (see Chapter 1). But I hope to rebut such a charge with the defence that the terms are being used just for convenience in exposition: it is the claims of individuals rather than stereotyped positions that will be criticised below.
24. I largely agree too with Hahn's criticisms (1973, p.5) of the larger view that "we have no criteria of true and false" in social science because "social science...must be 'political'". See Chapter 4, note 50, and Chapter 10.
25. Mill, On Liberty, Chapter 2. See Chapter 10, note 5.
26. See the end of Chapter 4 and the start of Chapter 9.
27. Perhaps some are already being enlisted. See below.
28. At the end of the Reswitching debate, for instance, Samuelson acknowledged that here he and other defenders of neoclassical theory had been in error. "If all this

causes headaches for those nostalgic for the old-time parables of neoclassical writing," he wrote in conclusion, "we must remind ourselves that scholars are not born to live an easy existence. We must respect, and appraise, the facts of life" (1966, p.582). But see note 29; and Ferguson's position, in Chapter 9.

29. Joan Robinson writes (1973, p.120) that "in the so-called reswitching debate...the neo-neoclassics had to admit that Sraffa was right. But:

He who is convinced against his will

Is of the same opinion still."

I would say that if he is of the same opinion, then he hasn't yet been convinced; whilst if the opinion now held is a rather different, modified one, it needs to be shown why, if at all, the new position must fail too. See note 28.

30. See particularly Chapter 7. And see below, on what might be salvaged if the hearsay evidence has misled.
31. Some of his work in international trade theory in turn serves as part of the evidence in Chapter 8.
32. See the note on Mill in Chapter 2 (note 10).
33. Even if the theory couldn't be demonstrated to be faulty, there might still be reasons for preferring an alternative one. But such reasons would be less likely to convince supporters of the theory of any need for change.

Chapter 7THE CURATE'S EGG

"I'm afraid you've got a bad egg, Mr. Jones!"

"Oh no, my Lord, I assure you! Parts of it are excellent!"

Punch, 1895.

Taxed with a wealth of evidence presented as showing that the assumptions of GE theory disagree with aspects of reality, defenders of that theory will rarely now try to insist that these assumptions are actually true. But they may well insist instead that the theory's conclusions are borne out - if not always, yet sometimes; and if not precisely, then still more or less.¹ Claiming this virtue in the theory, they would then lightly dismiss the non-fulfilment of its assumptions as a thing of little moment, not unsurprising in an abstract theory. Thus, in effect, recognising that the theory has (in their eyes, unavoidably) its "bad" parts, they would defend it by saying how "good" it is in others. Now there are theories for which defence along these lines might well sometimes succeed.² Rather similarly (to resort to an analogy), the judgement that parts of them are good may save some things to eat from going straight to the rubbish-bin - bananas, for instance, which might be bad at one end yet reasonably good at the other. Or trifles, or gateaux, with their various layers. Or crab: more clearly still, since the very best of crabs are bad to eat in parts. But with eggs it is different. If an egg is bad in parts, then we take it as bad right through (and we scrap it, hoping the next is good). This chapter will suggest that, as with the badness of eggs, so also with that

of GE theory.³ For whereas defenders of this theory, believing its conclusions to be true or nearly true, may still tend to speak with pride of the practical value of those parts that are deduced, I think it can be shown that their boast of empirical worth must be wry indeed: the boast that the theory is "good in parts" - like the curate's egg.

The empirical claims of the would-be defenders of GE theory have, of course, a familiar ring. Such defenders would be following Friedman in admitting with such equanimity that their theory's assumptions are not literally true. Some might even say these assumptions were not meant to be understood as anything more than approximations in the first place; and many might very well draw on Friedman's persuasive point that falsehood may be acceptable as an incidental effect of another process in theory-construction - of the necessary one of abstracting from the detail of complex reality. The theorist, they would claim, is bound to simplify and abstract, the success story of natural science illustrating how worthwhile his enterprise can be. But since no theory can hope to "tell the whole truth", theories must, the argument goes, be aimed at summarising or epitomising it; and if they are, then theorists may need to use assumptions which are not restricted to telling "nothing but the truth" either. In this way, the defenders, taking advantage of Friedman's ingenious case, might appear to shift any burden of proof onto those critics who used to denounce their theory for its neglect, or denial, of sundry matters of fact, challenging the critics now somehow to show that facts excluded from GE theory (and perhaps included in its competitors) are so important that they must be deemed essential ones.⁴ The

course of the Methodology Mystery illustrates, I think, how far "revolutionary" spokesmen have underestimated the subtlety and significance of this possible line of defence - a point pursued further in Chapter 9. However, the Mystery's solution has also highlighted the fact that there is a possible loophole in such a defence: it emerged in Chapter 4 that to show that falsity of assumptions does not always matter is not to demonstrate that it does not matter ever; and the excluded facts could, after all, in some way be essential ones. Chapter 5 has suggested that, for Friedman's business theory, matter the falsity does, there transmitting itself directly to the conclusions; so that, so far as assessing that particular theory on empirical grounds is concerned, Friedman's point about abstraction^{is}/~~a~~ distraction. Now, I am suggesting that the whole of general equilibrium theory is a theory in just the same boat - that the falsity of its assumptions too is sufficient to undermine any bid for factual strength in its conclusions.⁵

Could the conclusions drawn in GE theory be true even though the assumptions of that theory are false? Surely the answer is, no. It is economists from within the field of GE theory who have in recent years been working on tightening the statement of the conditions under which it could be held to apply,⁶ conditions some of which are held to be, as I understand it, necessary for the truth of the theory's conclusions.⁷ These economists allow that the conditions they are now able to state with such precision are extremely stringent ones: thus, for example, Hahn, in his recent inaugural lecture (1973, p. 27) notes that "tradition ... is forced ... to say that the economy is out of

equilibrium if a housewife finds on a rare instance that the shop has sold out of butter". They also themselves acknowledge that these conditions are not (and are hardly likely to be) fulfilled in the real world. Again, Hahn, in seeking to defend the "modern orthodoxy" attacked in a recent book by Joan Robinson, commits himself to the claim that (1972, p. 206) "'orthodox' economics is laying bare the exacting and implausible assumptions required for the neoclassical tradition". In the same review, he speaks (p. 205) of the "extreme vulnerability of the theory of a decentralised economy to matters of time and uncertainty", and says that "Debreu showed how that theory would require a large number of contingent futures markets and found that he could not then account for money. His work is the rod against which all claims for a competitive economy must be measured" (my emphasis). The same broad idea - that work in the neoclassical tradition is establishing that non-fulfilment of certain conditions is sufficient for the falsity of the conclusions of GE theory - appears in Hahn and Arrow's book, "General Competitive Analysis" (1971). In the preface, they pose the question "whether this enquiry into a [GE] economy, apparently so abstracted from the world, is worthwhile" and argue in answer that it is worth showing "just how the features of the world regarded as essential in any description of it also make it impossible to substantiate the claims ... made on behalf of the 'invisible hand' ... In attempting to answer the question 'Could it be true?', we learn a good deal about why it may not be true" (p. vii). The inaugural lecture (which in large measure abandons the traditional GE model)⁸ perhaps goes furthest of all. Hahn writes (p. 14) that "the

Arrow-Debreu equilibrium is very useful when for instance one comes to argue with someone who maintains that we need not worry about exhaustible resources because they will always have prices which ensure their 'proper' use ... a quick way of disposing of the claim is to note that an Arrow-Debreu equilibrium must be an assumption he is making for the economy and then to show why the economy cannot be in this state" (again, my emphasis). Hahn still thinks the theory may have a role, for he continues, "this negative role of Arrow-Debreu equilibrium I consider almost to be sufficient justification for it."⁹ Since theories in the neoclassical mould, but typically much less precise than Arrow-Debreu, enter so widely into the textbooks, then, in so far as propositions implied by such theories are or might be thought to be meaningful, I agree with Hahn (against Coddington's recent criticisms of this passage of the lecture) that the fact the Arrow-Debreu formalisation can play this negative role is significant. And Hahn does draw the moral I am arguing for when he adds that "for descriptive purposes of course this negative role is hardly a recommendation."

Strangely, few opponents of GE economics have made much of the fact that some of its assumptions are acknowledged to be both necessary for the truth of propositions deduced and false. Kaldor, however, is an exception. Noting (p. 1238) the revised statements of the theory's assumptions "forced on its practitioners by the ever more precise cognition of the needs of logical consistency", he first makes the claim (already met in Chapter 6) that work on relaxing them has not

yet begun. But he is also aware of the value of knowing that propositions associated with equilibrium theory have been "shown to be valid only on assumptions that are ... directly contrary to experience"; for he goes on to say (p. 1240) that "the pure theorist has successfully (though perhaps inadvertently) demonstrated that the main implications of this theory cannot possibly hold in reality", adding that "[however, he] has not yet managed to pass his message down the line to the textbook writer and to the classroom".

Is GE theory then in ruins, but for the negative role Hahn makes out for it - of giving an object lesson in structural weakness? Not quite. As Hahn was quick to point out (1973, p. 8), to put the case as Kaldor does is to neglect "the large literature on the 'removal of the scaffolding'". Putting the same point differently, though the fact that the necessary assumptions are false does mean conclusions drawn from them cannot be true, this does not tell us that the conclusions may not closely approach the truth; and the possibility of them doing so apparently remaining, possibilities for developing or "reforming" GE theory might seem to remain too: mightn't slightly more realistic assumptions be stated that could imply the true state of affairs?

Again, I believe it to have been established that the answer has to be no - established this time in "The General Theory of the Second Best". As I understand it, this important paper of Lipsey and Lancaster (1956-7) can be seen, in this context, as showing that even if the assumptions of GE theory were very close approximations to the truth, still this closeness could not be relied on to be "good enough": against

"reformist" claims, non-fulfilment of even just one of the assumptions (or replacement of just one of the necessary assumptions by a significantly modified assumption,¹⁰ however closely related to the one displaced) means that no general claim can be made to the effect that the facts (or the modified conclusions that could be drawn) come near to the traditional implications. This general result is surely both immensely significant for GE theory¹¹ and initially surprising¹²; and it seems worth quoting the Lipsey and Lancaster conclusions in some detail.

Because they were specially concerned with welfare applications of their theorem (the status of traditional welfare economics being, in the late 1950's, in question in very much the same way as that of the whole of GE theory is today), Lipsey and Lancaster's conclusions are for the most part cast in terms of the fulfilment of the Paretian optimum conditions. They describe their general theorem for a second best optimum, then, (pp. 11 and 12) as showing that "given that one of the Paretian optimum conditions cannot be fulfilled, then an optimum situation can be achieved only by departing from all the other Paretian conditions". Furthermore, it shows that "in general, nothing can be said about the direction or magnitude of the secondary departures from the optimum conditions made necessary by the original non-fulfilment of one condition". Then, "there is no a priori way to judge as between various situations in which none of the Paretian conditions are fulfilled. In particular, it is not true that a situation in which all departures from the optimum conditions are of the same direction and magnitude

is necessarily superior to one in which the deviations vary in direction and magnitude." And again, "there is no a priori way to judge as between various situations in which some of the Paretian optimum conditions are fulfilled while others are not. Specifically, it is not true that a situation in which more, but not all, of the optimum conditions are fulfilled is necessarily, or even likely to be, superior to a situation in which fewer are fulfilled" - a negative corollary expressed more colourfully by Mishan (1959) when he writes of it having been shown that "if one or more of the optimum conditions could not ... be met in one or more sectors of the economy, one did not make the best of a bad job by proceeding blithely to fulfil the remaining conditions."

But the general theory of the second best is concerned, as Lipsey and Lancaster themselves explicitly state, "not just with welfare theory" but rather with "all maximization problems". This fact is implicit too in Mishan's account of their theory: he speaks (p. 202) of the situation they treat of as one in which "some particular institutional or policy constraint prevented the realisation of all the conditions necessary for a true summit position" (my emphasis), and states that their conclusion "is proved elegantly by the simple mathematics of maximising a function of n variables subject to the usual constraints - such as the production function - plus an "artificial" constraint in the form of an inequality of one of the conventional marginal conditions". Amidst the special interest in welfare theory in the 1950's, the non-welfare applications of Lipsey and Lancaster's theorem caused relatively little stir; and they seem to have been passed over since.¹³ But their significance

now seems clear: the general theory of the second best is surely sufficiently general to apply to descriptive aspects of GE theory too.

Amongst the non-welfare applications of second best theory that Lipsey and Lancaster mention, one is of special interest here in view of the case Friedman puts in defence of approximation procedures. They refer to an article of Smithies in which ^{he considers} the case of a multi-input firm seeking to maximise profits where a boundary constraint prevents one factor being employed in the amount profit-maximisation would call for.¹⁴ "Smithies then shows", write Lipsey and Lancaster (p. 16), "that given the constraint, marginal cost does not equal marginal productivity for this input, profits will be maximised only by departing from the condition marginal cost equals marginal productivity for all other inputs. Furthermore, there is no a priori reason for thinking that the nature of the inequality will be the same for all factors. Profit maximisation may require that some factors be employed only to a point where marginal productivity exceeds marginal cost while other factors are used up to a point where marginal productivity falls below marginal cost". In this situation, behaviour that appears to approximate maximising behaviour as nearly as possible (equating marginal costs and revenues for all the, perhaps very many, other inputs involved) would lead to perverse results.

Thus, the theory of the second best shows that "it requires only that in one sector the conventional ... conditions be abandoned as impracticable for the conventional ... conditions to be irrelevant in the remaining sectors" (Mishan, p. 203); and it demonstrates too "the

extraordinary difficulty of making a priori judgments about the types of policy likely to be required in situations where the Paretian optimum [the maximum] is unattainable, and the second best must be aimed at" (Lipsey and Lancaster, p. 28). There seems to be a further moral, then, for the situation described above in which Hahn's single housewife cannot buy her butter, over and above the moral that this situation has to be counted, in the traditional theory,¹⁵ a disequilibrium one. For even if, let us suppose, 49,999,999 other housewives are obtaining all the goods (including butter) that they desire, and the 50-millionth housewife too buys everything but butter that she wants, we do not seem able to say, what would intuitively seem appealing, that this economy of 50 million people approximates an economy in equilibrium - or that it is more nearly in equilibrium than it would be if, the butter shortage being as before, some other housewife could not track down her margarine. When the GE assumptions are not fulfilled (and they never are), then even if they were to come near to fulfilment little of empirical weight seems capable of salvage from traditional theory¹⁶: sadly for "reformists", the particular nature of their theory itself precludes the easy claim that, though all is not "ideal", still things may in general rub along pretty much as if they were. This all-or-nothing state of affairs is apparently shared, as Kornai has pointed out, by the closed axiomatic system of (some of) modern physics: he quotes Heisenberg's statement that modern physical theory "cannot be corrected at all since, as a result of its system of axioms, it has become really a mathematical crystal, some rigid thing which may be correct or incorrect, but without an intermediate case".¹⁷

For GE theory, that might seem to be that. The stakes having been set at all-or-nothing, and hence the result on empirical evidence being nil, is there anything more to be said? I think there is. For I doubt if all defenders of GE theory would take the argument given above as, in itself, finally decisive against traditional theory. There is one last line of defence behind which they might hope to retreat, and traces of which can already be detected in the literature. Though acknowledging that the GE assumptions are false and even admitting irregularities in the idea of a merely "approximate" general equilibrium, might not equilibrium addicts still cherish their theory as at least representing a logical possibility that, though seemingly irrelevant now, may not always be so - as a design whose significance may be independent of its being implemented? May they not say that it is not just assumptions but conclusions too which can have a value though far abstracted from reality?

Chapter 8 will investigate this suggestion, asking: "If Economic Theory nowhere approaches Telling the Truth, can we be sure it is Really Referring?"

Appendix VIIFootnotes to Chapter 7

1. For another claim which some defenders of the theory seem to make - that its value in no way depends on any part of it being fairly faithful to reality - see below, and Chapter 8.
2. Such a defence would seem to help support, for instance, Friedman's example drawn from physics (a theory employing the "assumption" of a vacuum).
3. Does it follow that GE theory should be "scrapped" too? See below and Chapter 8; but also Chapter 6.
4. Chapter 9 will discuss in what ways some rival theories may be offering an advance in "realism".
5. See note 3.
6. Sraffa's paper of 1926, showing that the perfectly competitive situation is incompatible with an assumption of only partial and static equilibrium, perhaps marked a climacteric in the development of neoclassical theory.
7. I must confess that I would be chary of specifying here precisely which of these conditions are necessary (for precisely which articulation of the theory) - the more so when both Kaldor and Joan Robinson have been so roundly taken to task for getting the theory wrong (see Chapter 6). But I think it is sufficient here to take the theory at the valuation of a major practitioner of it, Hahn

- who also did the scolding.

8. Hahn writes, for instance, that although he thinks it "useful to have a concept of equilibrium states", he does "not believe the Arrow-Debreu notion to be the appropriate one" (p. 9). He does sketch the lines on which he thinks an alternative and preferable concept might be developed, but notes himself that this still leaves him "very much at the beginning of what could be called a theory" (p. 38), adding that more needs to be done "before even a tentative judgement on what has been proposed is possible". It would indeed probably be early days to venture any pronouncement on equilibrium theory in the new, and on the face of it very different, sense of equilibrium that Hahn is suggesting; but whereas Hahn has "some confidence in the main features of the story" (p. 38), I am left wondering how far one central feature, a concept suggested as being one of "learning" (see especially pp. 18 to 21) can legitimately so be described. (See Chapters 6 and 8).

9. Can the deduced propositions legitimately be counted meaningful ones? See Chapter 8.

Although Coddington's paper (1975, to be published shortly in a shortened version) drew my attention to the significance of this passage, it seems to me to give an entirely mistaken account of the point Hahn is making.

10. Taking a "significantly" modified assumption to be one that is changed in any way, provided only that the change is not a merely formal one.

11. Lipsey and Lancaster's paper did provoke an immediate discussion in the same journal, over the strength of the conclusions they were claiming; but their case has subsequently been accepted in the literature. It is given an important place, for instance, in Mishan's authoritative survey of welfare economics (1959), where the initial dispute is taken into account (and Mishan saw no need to qualify the place given to the paper when his survey was republished in 1965).
12. Lipsey and Lancaster remark at the opening of their paper that, although the main principles of the theory they go on to state in a general form "have undoubtedly gained wide acceptance ... the principles often seem to be forgotten in the context of specific problems and, when they are rediscovered and stated in the form pertinent to some problem, this seems to evoke expressions of surprise and doubt rather than of immediate agreement and satisfaction at the discovery of yet another application of the already accepted generalizations"(p. 11).
13. Boland's (1970) paper is unusual in suggesting that the Lipsey and Lancaster paper has importance for the literature on methodology in economics; but he too only speaks of their theory as applying in the welfare context.
14. More specifically, the boundaries that Smithies suggests there may be to the production function "take the form of irreducible minimum amounts of certain inputs, it being possible to employ more but not less than these minimum amounts" (Lipsey and Lancaster, p. 15);

and the situation envisaged is one in which profit maximisation calls for "the employment of an amount of one factor less than the minimum technically possible amount". Situations that are at least so constrained seem fairly likely to arise.

15. The situation need not be counted a disequilibrium one on the new theory of equilibrium that Hahn has sketched; but see note 8.
16. What, then, is the position of the literature allegedly on the "removal of the scaffolding"? At first sight, some of this might seem to provide counter-examples to the theory of the second best.

But consider a case Hahn discusses, that in which there are increasing returns in the economy. This seems to be an area which in Hahn would believe that some of the scaffolding can go. What he is actually able to say seems, however, to illustrate the second best theorem. On the one hand, he writes (1973, p. 12) that "when there are increasing returns it may not be possible to show that there are any logically possible economic states which qualify as ... an Arrow-Debreu equilibrium". On the other, he goes on to say that "an Arrow-Debreu equilibrium may exist when there are increasing returns". In other words, in the increasing returns case, we cannot in general say whether such an equilibrium exists or not: we are in, it seems, a second best situation. Hahn then elaborates, considering cases where "we have particular information about the relationships characterising the economy", or later, specifically, where "in a precise sense increasing

returns to scale are small relatively to the scale of the economy" (p. 13) - cases in which firm conclusions can be drawn. But this is still, I think, consistent with second best theory, which allows that specific, though by no means necessarily simple, conclusions may be reached piecemeal where the requisite detailed information is available. The position seems to remain a negative one.

Given increasing returns, it will not be possible to say a priori whether an Arrow-Debreu equilibrium exists, a fact that can be known only when "precise" detailed information is known too; and equilibrium theory of the traditional form seems to have lost any claim to be "general". (And of course, those specific cases in which the necessary conditions for an equilibrium to exist can be known, may very well be ones that are no more realised than is the unattained "first best" case.)

17. I wonder if Heisenberg is thinking of Einsteinian theory, as expressed in a system of interdependent equations?

It would be interesting to explore the question how far there are general reasons for theories being "all-or-nothing" ones.

Chapter 8REALLY REFERRING IN ECONOMIC THEORY

or

THE CHESHIRE CAT'S GRIN

"Military officers destitute of military knowledge; naval officers with no idea of a ship; civil officers without a notion of affairs ... all totally unfit for their several callings, all lying horribly in pretending to belong to them."

Dickens, A Tale of Two Cities

"The Cat ... vanished quite slowly, beginning with the end of the tail, and ending with the grin, which remained some time after the rest of it had gone."

Carroll, Alice's Adventures in Wonderland

In 1964, answering Machlup, Samuelson wrote of his own factor-price equalisation analysis: "When one looks at the real world, one finds it obvious that the hypotheses of the syllogism are far from valid; and also, the consequences are far from valid. This is indeed a matter for regret and full disclosure of inaccuracy should be made. Nevertheless ... a strong polar case like this ... can often shed useful light on factual reality" (1964, p. 737). Rather than arguing, then, that in some way this particular part of neoclassical theory "approximates" reality, he instead admits that not only its assumptions but its conclusions too are "far from valid"; pleading, however, that though they are not valid, it's well worth considering how things would look if they were. More recently, Koopmans (1974) has taken a similar line. He too is sanguine about the value of equilibrium theory in its strict form,

writing: "Does the model of competitive equilibrium in its simplest form represent one pure and special case, one valuable foothold for a steep climb? My answer: yes"; and adding, by way of explanation, "I do not hesitate about my 'yes' ... because of the value to economic theory of a fully worked out special case". But for all Koopman's own lack of hesitation, shouldn't we pause to consider what it is that this so "special" case can be a special case of?

For one begins to wonder whether there may not be in the offing a general defence to the effect that the very fact of the non-realisation in the past, present or foreseeable future of the assumptions and conclusions of GE theory somehow adds significantly to its purity as an ideal. To the now familiar claim that the theorist must simplify and abstract there is now added the view that the more he abstracts, the better. Lancaster, introducing a surely extremely abstract model of international trade¹, writes (1957 (1969), p. 50) that, though the theory has been attacked for its "unrealism", it is "indeed, strengthened by the very properties which have been subject to so much criticism", being "the simple model of international trade when things are reduced to most elemental terms". Then he adds that these terms are "not necessarily the most elementary terms", perhaps suggesting implicitly an aspect of this defence that is made explicit in Hahn (1973) - that it is because the highly abstract can be difficult to comprehend, rather than for any better reason, that it so often becomes the target for attack. "Here is what Russell has to say" says Hahn (1973, p. 3): "'Many people have a passionate hatred of abstraction, chiefly, I think, because of its

intellectual difficulty; but as they do not wish to give this reason they invent all sorts of others that sound grand. They say that all reality is concrete, and that in making abstractions we are leaving out the essential. They say that all abstraction is falsification, and that as soon as you have left out any aspect of something actual you have exposed yourself to the risk of fallacy in arguing from its remaining aspects alone. Those who argue in this way are in fact concerned with matters quite other than those that concern science ... It is characteristic of the advance of science that less and less is found to be datum and more and more is found to be inference'. "I happen to believe", writes Hahn, "that what [Russell] is here saying applies to our subject."

Yet, with Worswick, "one cannot avoid some unease"². If we accept Friedman's point that one may legitimately abstract beyond the point of literal truth, does this mean there are then no limits on how far the process should go? For surely GE theory does seem to err in the direction of over-abstraction, eventually reaching, one might very well feel, vacuity.

Kornai (1971) has criticised GE theory for the "error of uniformization": he writes that "the GE school makes the description of economic systems entirely too dull; it over-schematizes and impoverishes it ... recognising only one type of consumer behaviour, one type of motive force for the firm, and one type of information"; thus, "for the economist of the GE school, 1 seems to be the magic number". And a glance at neoclassical texts might suggest that Kornai is right. A section of Lancaster's paper, for instance, is devoted to demonstrating

geometrically "the equivalence between a pair of countries engaged in trade and a single economy, under certain assumptions ... a pair of countries, with the properties which have been analysed in this paper, is exactly equivalent to a single country whose endowment of labour and capital is equal to the sum of the endowments of the two individual countries" (1957 (1969), p. 63). But does Kornai go quite far enough? Here is Hahn admitting that "traditional equilibrium theory does best when the individual has no importance - he is of measure zero" (1973, p. 33).

A number of critics of traditional GE theory have appealed to the fact that it seems to abstract entirely from some particular factor, whilst yet purporting to treat of it - with resulting confusion of terms. Joan Robinson, for instance, has made this point frequently, about the theory's treatment of capital³: for the "neo-classics", she states, "Capital consists of some homogenous physical stuff. Professor Meade called it steel. I said, let us call it leets because we do not know what it is" (1973, p. 126). Her question, "When is capital not capital?" points the issue. Others have asked, in effect "When is a firm not a firm?" Thus Archibald, in his introduction to a recent set of readings in the theory of the firm, writes (1971, p. 10) that "in neoclassical general-equilibrium theory, firms are completely described by their production functions" and that "the formal theory of general equilibrium is extremely difficult to handle without important simplifications, particularly perfect competition. But in perfect competition firms have so little to do, particularly in the absence of technical change and

uncertainty, that there is nothing worthy of a separate title." Essentially the same point is made by Latsis (1972, p. 210), when he says: "The neoclassical approach may perhaps be fairly termed as envisaging entrepreneurs without entrepreneurial functions ... decision makers without decision procedures". And indeed Hahn too expresses some arguments of a similar form: for instance (1973, p. 12), he admits: "it now seems to me clear that there are logical difficulties in accounting for the existence of agents called firms at all unless we allow there to be increasing returns of some sort. But when there are increasing returns it may not be possible to show that there are any logically possible economic states which qualify as ... an Arrow-Debreu equilibrium".

Indeed, Hahn goes further and applies the argument-form more generally: "we want ... our equilibrium notion to be sequential in an essential way ... This ... requires that information processes and costs, transactions and transaction costs and also expectations and uncertainty be explicitly and essentially included in the equilibrium notion. This is what the Arrow-Debreu construction does not do" (1973, p. 16)⁴. So does Joan Robinson: she claims (1973, p. 126) that the neo-neoclassics made "output also consist of leets - they reduced the whole argument to a 'one-commodity world'. The use of models in economic theory is to eliminate inessential complications from the analysis of some problem so as to concentrate on the main point; the use of this model is just to eliminate the point." (Presumably the point eliminated here needn't just be a point about capital; for who are these men who can live by leets alone? If "we do not know what [they are]",

shouldn't we write them backwards too, to be on the safe side, and keep to a model about nem?) Or again, she writes (1973, p. 132) "it is not legitimate to say: Let us first assume perfect competition and bring in the complications later; for an economy in which text-book perfect competition was possible would be different from our own in important respects; we do not know what contradictions we may be letting ourselves in for by assuming it."

But it is Marx who seems first to have articulated an argument on these lines against equilibrium theory; and his expression of it is perhaps the most explicit on abstraction too. In The Grundrisse, he puts it thus: "Free competition has never yet been developed by the economists, no matter how much they prattle about it. It has been understood only negatively: i.e. as negation of monopolies, the guild system, legal regularities etc. ... But it also has to be something for itself, after all, since a mere O is an empty negation, abstraction" (p. 413). He goes on to say that "the reduction is not even formally scientific, to the extent that everything is reduced to a real economic relation by dropping the difference that development makes; rather, sometimes one and sometimes another side is dropped in order to bring out now one, now another side of the identity": for instance, after defining wages and profit in such a way that they "are identical ... it is ... an error of language to call one payment wages, the other profit". (p. 250)

By now, it will have become clear what form my argument against the final retreat⁵ of traditional equilibrium theorists is to take. For it seems to me that the arguments above can be seen as sharing a

common transcendental⁶ form; whilst the later quotations suggest that an argument of such a form applies unfavourably not only to some individual part of the GE system (when defenders could perhaps attempt to plead that this part is not essential to the working of others)⁷ but to the whole theory. I am suggesting then, that the claim of some defenders, that GE theory in its purest form represents an instructive possible world, falls foul of a Transcendental Argument against the Too Abstract (TATA).

The burden of the argument is that the supposed "special" or "polar" case of GE theory is too special to be a case at all - that the proposed design for the GE edifice not only could never be implemented but also even on paper does not truly take shape. For the GE assumptions are so stringent that strict fulfilment of them surely implies non-fulfilment of preconditions for various forms of human reasoning (a fact that may go unnoticed because in the real world we become so accustomed to taking these preconditions for granted). For instance, Joan Robinson has suggested that the GE assumptions conflict with our real-world concepts of time and space; but, if they do, can even our customary laws of logic be relied on to hold in the truly GE world? Again, other real-world considerations about the working of the human understanding can be used, I think,⁸ to set broad limits on what can count for us⁹ as comprehensible alternative worlds - limits which would be violated in a strictly GE system. Just as the physical possibility of humanity experiencing a different world depends on that world having characteristics that will support human life, so the rational possibility of our

comprehending an alternative world depends on that world having features that permit us to grasp it¹⁰ (and without which it can't really be counted as an alternative world at all). Hollis (1967-68) has an example, designed to expose the difficulties in conceiving a Humean world of unstructured sense perceptions, in which increasingly unpredictable change caused by a capricious gale leads eventually to a situation lacking discriminable regularities and excluding any possibility of referring to objects or predicating properties of them. By contrast, in the GE world, it is the excess of regularity, the "error of uniformization", that would defeat all attempts to discriminate, to refer and to predicate: extreme uniformity would prevent us from picking the now all too continuous things out from each other.¹¹ And whereas for Hollis's world, the descriptive categories of any possible public language would be too few (generation and acceptance of new words inevitably failing to keep pace with events), for the GE world they would be too many: with all redundant terms eliminated, too few would be left to form a language. In sum, the GE world is one in which there could not be human agents, thinking and communicating; and if we seem able to imagine any men rather than nem in it, this is only by means of illegitimately importing features of reality.¹² But since the categories of our ordinary language lack application in a GE system, failing to refer, and since that system nonetheless relies on these terms for its expression, it is surely logically incoherent: even as a parable, the theory doesn't make sense.

Not only, then, does discordant actuality defy explanation in

terms of GE harmonies, but also, in presenting its own never-never land as having a distinct form at all, GE theory has in fact to depend on terms wrested from the language of reality - terms that, like fish drawn out of water, can't long persist alive. As the Cheshire Cat's grin to the rest of the Cat, so empty names of worldly things have lingered after the rest of the world has vanished.

Perhaps defenders of GE theory might here protest that the TATA could only be held to have force against an overstrict statement of their theory. They might say that they do not need critics to tell them that, for instance, firms are not so alike as the strict statements of the theory suppose them to be; and they might claim they have always been willing to recognise that in applications of the theory there will be differences to take into account. There are idealisations in the theory, it is true (they might continue), but this does not mean the theory must fail to refer; for when its assumptions are relaxed, be it only a little, the theory may describe how things really are. But such a plea takes the defenders back to the problems discussed in Chapter 7.

In this way, the TATA complements the case of Chapter 7, based on the nature of the damage done when the GE assumptions are not fulfilled, by drawing on the peculiarities of any situation in which they were. If the arguments are valid, then on the one hand, the defenders of GE theory cannot correctly claim that although its assumptions are not fulfilled still its conclusions have general empirical worth; and, on the other, they cannot legitimately appeal either to the idea that their theory still has value in mapping out a merely possible world.

Thus GE theory tells us neither what is nor what almost is nor even what might conceivably come to be. Because of this, if theorists now move on to the development of theories that do come down-to-earth, they need not, I think, look back regretfully. Perhaps they will not, and perhaps they need not, forget the paradise they dreamt of; but still this paradise lost was never more than an illusion.

"They looking back, all th^e Eastern side beheld
Of Paradise, so late thir happie seat ...
Some natural tears they drop'd, but wip'd them soon;
The World was all before them, where to choose
Thir place of rest, and Providence thir guide:
They hand in hand with wandring steps and slow,
Through Eden took their solitarie way."

Milton, Paradise Lost

Appendix VIIIFootnotes to Chapter 8

1. See also pp.8.3 to 8.4.
2. Based on Worswick (1972, p.74). (See page 6.5).
3. She extends the argument beyond capital too: see below.
4. And so, Hahn judges the Arrow-Debreu equilibrium concept inappropriate, and sketches his alternative one. See Chapter 7, note 8.
5. Not counting the appeal to faith. (See Chapter 9).
6. Very roughly, they argue that neoclassical assumptions contradict conditions that are necessary for our very experience (and presupposed in our concepts) of economic factors.
7. On the face of it, defenders of rival theories might need to plead thus too; but see Chapter 9.
8. As Strawson (1959) and Hollis (1967-8) have used them.
9. See Appendix D.
10. Our being able to imagine a GE world, after a fashion, would not be sufficient to make it a comprehensible one; for equally, we seem able to imagine, after a fashion, the incomprehensible. See below and note 12.
11. Defenders of GE theory might here suggest that this extreme uniformity is shared by some abstract theories in natural science, which we yet have accepted. But I wonder how far uniformity there is so extreme, and whether, where limits are invoked, for instance, they apply just one at a time.

(What, otherwise, does happen when an irresistible force meets an immovable mass?) Also, the extreme assumptions of natural science can, I suspect, more easily be relaxed.

12. Similarly, Hollis says (1967-8): "we can apparently imagine a total chaos...we can think away every feature of an ordered world in turn, but this does not mean we can understand a world without any features left. On the contrary, if private consciousness provides a standpoint for describing total chaos, it does so only in so far as it gives us a private world with inductive stability, which we then deny total chaos to resemble...total chaos remains a limit and not a possibility." (p.278)

Chapter 9GLASS HOUSES?

"What do you get by throwing stones at your enemy's windows, while your own children look out at the casement?"

Secker, 1660

"Placing reliance upon neoclassical theory is a matter of faith"¹ one of its defenders has judged, when confronted with a logical difficulty in that theory. Will the faith of all defenders prove so strong? There is perhaps already some religious air about the talk of "shedding light" and of "climbing steeply" (a ladder to Heaven?)²; but still it does not seem to me that a general situation in which "criticism can have no effect"³ has yet been reached (or necessarily ever will be)⁴. And if the critical arguments of the past two chapters are not mistaken, together they must surely form a powerful pair.

It is in a combination of just such arguments, however, that Hahn scents paradox. "I want to emphasise...the paradoxical position of some of the critics", he writes (1973, p.13): "They complain of the excessive generality of the /equilibrium/ construction but at the same time believe that the whole edifice must tumble if it ceases to be completely general". But does any impression of paradox that there is here truly stem from those critics?⁵ I have been arguing, in effect, that it does not, but rather arises from the nature of GE theory itself, whose very peculiarity it is to be in precisely the anomolous situation being criticised. GE theory is a theory which indeed cannot stand unless it's general, yet

if general, is too general to stand; but it need not be paradoxical to point to this fact, for surely there may be theories that escape this choice between the Devil and the Deep Blue Sea.

So I join in both the "belief" and the "complaint" that Hahn has held to be jointly a paradoxical pair - claiming, against him, that the two are more effective working in harness. It is rarely, however, that "revolutionary" spokesmen even hint how these twin criticisms might mesh together, a failure that surely weakens their apparent basis for espousing rival theories.

Ellman, for instance, has suggested that GE theory is to be criticised for a "lack of correspondence between the theory and the economies we seek to analyse" (1972, p. 1480).⁷ But then, how much better can "revolutionary" theories fare? Can labour, for instance, really legitimately be treated as homogeneous, even though capital cannot? Or again, take Eatwell's (1974) recent exposition of Sraffa's alternative analysis. Eatwell is frank about the fact that the "composite commodity" introduced "in which wages are expressed, is a rather extreme abstraction." Then he continues "it is not, of course, assumed that the worker actually consumes the standard commodity, merely that the wage may be expressed in those terms... although an abstraction, the standard commodity represents all the essential characteristics of the actual organisation of production..." Doesn't this amount to saying that, although there is in fact no such "standard commodity", still we are justified in proceeding as if there were?

Moreover, consider Joan Robinson's case. It has not gone

unnoticed with Hahn (1972, p.206) that she is willing to admit "facts [that] are stylized with a vengeance". She herself acknowledges (1973, p.267) that in the simple Keynesian model, where money prices are assumed away, Marshall's "little question" of value ("Why does an egg cost more than a cup of tea?") disappears. She is explicit too about the Ricardian method of "taking strong cases", writing (1973, p.251) that "this means: swing your variable over a wide range and look at the two ends before you look at the middle". But then how do Ricardo's "strong cases" differ from the GE theorists' "special" or "polar" ones? The amplification she gives is unhelpful here; for she continues: "there is an art in doing this, it is not just a mechanical trick. What is a wide range in relation to the question in hand? The trick anyone can learn, but the power to recognise a wide range is a gift of God." There follow examples (p.252); but these serve to point the puzzle without offering guidance on how it is to be solved. For instance, "for Marx the strong case (for accumulation) is zero accumulation...you might think it rather a funny idea to study accumulation in terms of a system that is not accumulating. But if you think that, it just shows that you did not go to one of the best schools, and I will not be so snobbish as to rub it in." And again, "Keynes starts in a Marshallian short period. It certainly does seem rather odd, at the first glance, to assume zero accumulation when the very things you are going to talk about - saving and investment - are two aspects of accumulation. A number of smart Alecs have noticed this anomaly and then spent a lot of time pointing out the fundamental logical contradiction on which the General Theory

is based": here "Professor Kahn [made] an endeavour to explain what Keynes was doing"; but "this was in oral discussion, not published". What are those who missed the oral discussion then to make of the remark elsewhere (p.63) that "the Keynesian^e revolution brought us down from the neoclassical cloud-cuckoo-land, to here and now, facing the problems that we actually face"?

At this point, one might begin to wonder whether those rejecting GE theory as too abstract must, for consistency, jettison the current rivals too. It seems to me that perhaps they need not, when their rejection is based on the twin lines of argument given above. For whilst these arguments, if correct, provide a strong case against GE theory, they yet suggest how other abstract theories might be defended. They bring a case that Marxist and Keynesian^e theories could be proof against, if their advocates could show that the abstract assumptions of their simple versions are ones that can be relaxed.

Joan Robinson tells us that in the simple Keynesian^e model "Marshall's cup of tea dissolved into thin air". Why exactly does she think this mattered less than dissolving capital into leets did? "You assume away the complications till you have got the main problem worked out", she continues. And I am adding: But you need to know that, afterwards, you can⁸ bring them back.

Appendix IXFootnotes to Chapter 9

1. Ferguson (cited by Joan Robinson) responding to the reswitching argument.
2. See pages 8.1 and 8.2. See also p.5.13.
3. The conclusion Joan Robinson reached (1973, p.154) in her criticism of Ferguson's book.
4. Perhaps this is a matter of faith with me. But for some basis for the view, see pages 6.7 to 6.8, and especially note 28.
5. I am aware of few critics who have articulated the criticisms in this way (Joan Robinson is one; but see Chapter 6, note 22). See below.
6. It might be wondered whether the combination of Chapters 7 and 8 above is paradoxical because, whereas the TATA of the latter shows the propositions of GE theory as incoherent, the former relies on their coherence in claiming them to be false. (See page 7.5). But again, I think that any paradox stems from the position being criticised.
7. He is comparing criticism of this "lack of correspondence" with that of "the internal consistency of the theory being realised", seeing the two as unrelated.
8. Here, Joan Robinson also says (1973, p.268): "The price level comes into the argument, but it comes in as a complication, not as the main point" - allowing for the reintroduction

of complications. But neither the significance of being able to bring the complications back nor the reasons for differences in ability to do so between GE and rival theories seem to emerge clearly from this epigrammatic passage.

Chapter 10AN END AND A BEGINNING

Breathless with excitement at the challenging issues that have seemed to appear with each step along the way, I am very much aware that reaching this first destination in a sense only shows how much further one might eventually seek to go. A few problems may have been solved, some more at least defined, and several others touched on; but as many more are only beckoning from beyond the path. Perhaps this is inevitable when the track has lain at first through a maze of by-ways and later through almost uncharted territory.¹ Perhaps too it is a hopeful sign for the newly-named Philosophy of Economics; for the new areas do not seem to have proved barren, and, with unknown regions yet to venture into too, it should be good to go on exploring.²

What might the explorer hope to find? Perhaps the answers to two questions implicit in the route so far. Firstly, that of how far the dismal science resembles others.³ (Are there any economic laws; and if so, what are they like?⁴ Or is the economist rather treating of experience that, as Neale has said of history, "never repeats itself but ... offers analogies"?) Secondly, that of what relationships there are between capitalism, liberalism and particular economic theories. (When GE theory is an illusory monolith, but the capitalist economy can be tinkered with, can the link between the two be as close as has been supposed?)⁵

Or perhaps he might linger to consolidate past gains.⁶ Or might search for a bridge between the Is-Ought controversy and developments in welfare economics.⁷ Or take up the cause of Cause in economics.⁸ Or branch out in a dozen different ways.

Writing in 1939, the year after Harrod's Presidential Address, D.H. Robertson was apologetic about touching "on the distasteful subject of methodology": writing today, mightn't he, as I do, find it an intensely exciting one?

Appendix XFootnotes to Chapter 10

1. On a "chart" which has just been published, see Appendix D.
2. See Chapter 1.
3. MacDougall (1974, p.777) takes a common line when he claims that "it is blindingly obvious that we shall never achieve anything like the certainty of the natural sciences"; and he includes in his reasons for saying so, that "we cannot construct the same kind of controlled laboratory experiments." But is it so blindingly obvious that economics is doomed to an inferior position? Perhaps the dismal science will always be a poor sister to the natural sciences. On the other hand, it seems possible too that asking different questions might bring a gain in the certainty of economic knowledge (see note 4), and that some decisive experiments might be devised.
4. I fancy that some constraints on economic behaviour might usefully be thought of as economic laws. On the economics side, it would be interesting to consider from this point of view both the national income identities and relationships of sectoral balances in the economy; whilst on the philosophy side, this idea might link with the individualism/holism debate (along with economic aggregation problems), a recent^{oral} paper by Ryle, work on circular arguments and on the nature of necessity. See also note 8.
5. It has been implicit at some points in the thesis that liberalism (for example, on "thought and discussion",

with Mill) is compatible with economic doctrines other than neoclassical ones - a view that Friedman (1962) has disputed. Here one might consider, *inter alia*, how the arguments of Chapters 7 and 8 would relate to theories cognate with the GE one in other disciplines (e.g. theories of justice and formalised theories of democracy).

6. For instance, Chapter 4 and Appendix B suggest that considering accepted routes of approximation, summary and abstraction in more detail might prove worthwhile. Also it would be exciting to track down any parallels between various all-or-nothing theories (Chapter 7). And work on Lockean Abstract Ideas and on Bradley might complement Chapter 8.
7. This may be one of the points at which developments in economic theory might have value for philosophy.
8. It seems clear that the work of Lucas (1962) and of Mackie (1965) on cause has very important implications for econometric analysis (and doubtless for economic theory too).

Appendix A"Methodology" Misunderstandings

This appendix gives a brief summary of the principal misunderstandings of "The Methodology" that have appeared in the literature discussing it. It does not cover the wider economic literature, into which many, and sometimes cruder, misunderstandings have found their way too.

Since only misunderstandings are reported, some papers in the literature of the Friedman Affair get no mention at all (e.g. Archibald's review-discussion (1959) of Koopman's book (1957), with its two points of congruence with Chapter 5 above (pages 5.11 and 5.12)); and those that are mentioned gain next to no acknowledgement of any fair criticisms they also offer.

The areas of misunderstanding are listed separately, though some have tended to go together. It has been held that:

- (a) Friedman believes theories must and can be judged only by their predictions; and his methodology is at fault because this rules out considerations which ought to be taken into account too, and in particular ability to explain.

Rosenberg (1972) is perhaps the worst offender here. He has Friedman making the "sweeping" claim "that theory is to be judged exclusively by its predictive power for the class of phenomena which it is intended to explain" (p.17) and later (p.19) that "the only measure of satisfaction is predictive success". And he goes further; saying (p.17)

that the criteria of "explanatory power, simplicity, and economy, synthesis of disparate phenomena, consistency with other accepted theories, and the truth of ... axioms... cannot be dispensed with by mere assertion". Later (p.23) he recognises Friedman's "other grounds" and costs-against-accuracy remarks, but says on the first "what other grounds of equal acceptability are there...if predictive success is the only ground, as Friedman claims", and on the second that "this criterion conflicts with the general criterion of predictive success by importing economy and simplicity as criteria for the acceptance of theories".

Coddington (1972, p.3) associates Friedman with the view that "the characteristic of a theory which alone is relevant in appraising it as a contribution to economic knowledge is its predictive performance"; and although he does mention the "symmetry" view of explanation and prediction, he sees Friedman's account as "quite inconsistent with explanation or prediction invoking 'causes'" (p.3, n.14). And he thinks that Friedman's position is "better described as an extreme form of pragmatism or instrumentalism...[than] labelled as positivist" (p.2). Bear and Orr (1967) see Friedman as holding the instrumentalist view too (on Popper's definition). They share the view that "The Methodology's" methodology is contrary to pursuit of explanation: "A scientist is concerned with how things happen, not only with what happens, and the Friedman methodology makes it impossible effectively to pursue that concern" (p.191). Or, to go to Melitz (1965), "the 'as if' construal of economic assumptions" leads to answering only "the question

'how possibly' rather than the question 'why'" (p.50).

And in 1959, Rotwein noted Friedman's treatment of explanation and prediction as alike, but made no mention of the support of some philosophers for this "symmetry" view.

Needless perhaps to add that Klappholz and Agassi - and Wong - misunderstand here too.

(b) Friedman has failed to realise that simple assumptions are different from false ones; and believes that, for assumptions, "any old falsehood will do".

Thus, Rotwein takes the view that, when Friedman's "descriptive falsity" passage is seen as an argument for abstraction, it "conflicts with the procedure supported in his main thesis because there 'descriptively false' has its more direct and usual meaning: that is, we are told there that in testing the validity of a 'theory' it makes no difference whether or to what extent its 'assumption' does falsify reality"(p.565). Rather similarly, Rosenberg.

Cyert and Grunberg (1963) write (p.308): "Friedman's reference to abstract propositions as also 'unrealistic' in the sense of the statement about the billiard players confuses two quite different things: neglecting certain attributes of reality and making observably false statements about reality...an abstract proposition cannot refer to all attributes of the elements of the particular class, but it may not contain empirically untrue references." ("May not" here meaning "is not allowed to"?)

Perhaps likewise, Bear and Orr, and Melitz.

(Incidentally, Cyert and Grunberg also say (p.308,n.14) that "in his entire argument Friedman mentions only confirmatory evidence which he describes as 'dramatic' and 'conclusive'", objecting that "confirmatory evidence...merely indicates that a hypothesis has so far not been disconfirmed". Compare with Friedman, p.9 (as elsewhere in his argument): "The hypothesis is rejected if its predictions are contradicted... Factual evidence can never 'prove' a hypothesis; it can only fail to disprove it, which is what we generally mean when we say, somewhat inexactly, that the hypothesis has been 'confirmed' by experience".)

(c) Friedman has confused "assumptions", meaning "statement of initial conditions", with "assumptions", meaning "higher level hypotheses".

Barr (1971) writes that Friedman "talks about assumptions (i.e. ideal conditions) of hypotheses and assumptions that are hypotheses"(p.268). According to Barr, Friedman indicates in the vacuum example that the assumption of the hypothesis is that the buoyancy of air (and etc.) equal zero, whilst the hypothesis is summarised in the formula $s = \frac{1}{2}gt^2$; whereas in contrast, in the economics case, "he refers to the assumption as the hypothesis: the assumption that firms seek rationally to maximise their expected returns is labeled by Friedman as the 'maximisation of returns hypothesis'". But surely Friedman distinguishes between the actual maximisation of returns and deliberate maximising behaviour, with his "as if" hypothesis only "hypothesising" the former.

Likewise, Rosenberg; perhaps Clarkson (1963); and, in rather different terms, Melitz, and Cyert and Grunberg.

(d) Friedman holds that businessmen are DRRMs, billiard players do solve the relevant equations, and etc.

Melitz misinterprets the "as if" construction (see Appendix C). He writes (p.50) that "if businessmen act only as if trying to maximise profits, then evidently they do not exactly try to maximise profits...As a result, no specific conclusions about business's actions, however vague and tentative, can be strictly derived from the statement. In basing any prediction on the assumption of profit maximisation, it may safely be concluded that there is implicit reliance on the declaration that...businessmen really and truly try to maximise their profits...Broadly speaking, in using the economic postulates to explain and predict, we commit ourselves to what they say about the world, and thus it would seem almost mandatory to interpret them accordingly as straightforward declarations of fact".

Rosenberg holds, inter alia, that Friedman rejects the questionnaire evidence against the hypothesis that businessmen are DRRMs.

And Cyert and Grunberg give a remarkable account of the billiards example. Taking the hypothesis to be that expert billiard players make their shots by solving mathematical problems, they claim that Friedman does not consider this disconfirmed and consequently neither discards it, nor retains it (because of its correct predictions in a limited zone)

until a better hypothesis can be formulated. Rather, he chooses the alternative of splitting the hypothesis into two: "a hypothesis b_1 (expert billiard players solve the relevant mathematical problems before making a shot) and b_2 (expert billiard players have mathematical training to solve the mathematical problems presented by the game). Whereas b_2 is now considered to be strictly disconfirmed, b_1 is considered to be true." (p.307)

(e) Friedman misunderstands elementary logic.

Clarkson believes that Friedman's case depends on taking success of predictions as, in itself, confirming the propositions that imply them.

Rosenberg presents the twin of my argument that, where truth of assumptions is necessary for their conclusions' truth, falsity of the former will lead to falsity of the latter. The point he makes is that, with the truth of an assumption sufficient for the truth of its conclusion, to establish the truth of the former is to establish the truth of the latter. This does go against Friedman's claim that independent tests of assumptions are not relevant, taken as a general claim; but is not well-gearred to Friedman's specific concern with the status of tests of assumptions when these (simplifying) assumptions are often false. Rosenberg also makes the Samuelsonian point that "every theory implies its own assumptions, so if these are tested and confirmed, the theory must be confirmed to that extent." (p.18) It is true that assumptions imply themselves, but Rosenberg neglects

Friedman's anticipation of this objection (which De Alessi (1971) has drawn attention to). Recognising some possibility of interchange of assumptions and implications in different uses of a theory, Friedman allows that in these circumstances a hypothesis invoking false statements may be acceptable for some purposes but not for others. False assumptions will not have true implications when used to predict themselves - but they can have other, less trivial, uses. (Criticism of "The Methodology" on the grounds of interchange possibilities between assumptions and conclusions is related, I think, to Leontief's point (1971) that the "givens" of today become the unknowns of tomorrow; and to Rotwein's argument).

And Rotwein ends with "it is difficult to argue consistently in support of the unintelligible" (p.575); whilst Rosenberg offers the conclusion that economists "would do well to abandon Friedman's position for almost any other".

Appendix B"Descriptive" falsity

What does Friedman mean by his provocative claim in "The Methodology" that "to be important ... a hypothesis must be descriptively false in its assumptions" (p. 14)? Some commentators take him to be claiming that having false assumptions is a (or even the) prerequisite for the importance of a hypothesis; whilst others protest that, on the contrary, the context makes it plain that the word "falsity" in his claim is a misleading one, since he is not really talking of falsity here but only of descriptive incompleteness (simplicity, abstraction). But it is suggested in Chapters 3 and 4 above that neither of these versions of the claim may represent Friedman's position adequately. Certainly, the immediate context does show that he is discussing the importance of abstracting from the mass of descriptive detail that could in principle be given about phenomena (and other passages support this interpretation: see, in particular, his remarks about a theory of the wheat market that included details even of colours of the traders' hair and eyes (p. 32)). And Friedman also says (p. 15) that he has expressed his point "paradoxically". On the other hand, his examples and parts of his argument elsewhere surely make it plain that he is also especially concerned with stressing the role to be played by assumptions that are false. Chapters 3 and 4 argued, then, that what Friedman seems to be claiming is not only that just the key facts about X (for a particular predictive (explanatory) purpose) should be included, but also that stating just, as it were, the essence of the truth about X

(for that purpose) may (in our present state of knowledge) itself involve stating a falsehood.

The idea that broadly successful attempts to epitomise detail may lead into falsehood (Friedman's idea, on my interpretation) has not been very widely recognised, I think, in writings on economic method. There seems to have been a fairly general presumption (though not all hold this: see below) that a process of excluding unwanted details can lead to statements as simple as could be wished without ever trespassing into falsehood, and that taking some licence with strict truth has no connection at all with the ability to sum things up simply. (I wonder how aware Kaldor is of the possibility that many, on the face of it valuable, abstractions might be counted as literally false, when (1972, p. 1240) he writes of "assumptions that are manifestly unreal - that is to say, directly contrary to experience and not just 'abstract'." But see below).

Yet the idea that I am taking to be Friedman's does perhaps deserve some credence. An article by Barr (1971) on idealisations in science seems to give some support for the idea (although Barr believes, I think, that Friedman is abusing it). Barr makes it part of his analysis of an ideal condition (holding ideal conditions to be involved in many idealisations in the natural and social sciences) that such a condition is "a formula in which occur state variables whose existential closure is false" (p. 271). He also suggests that there may be "no major semantic difference between (most) other universal laws and laws in idealized theories" (p. 266), referring here both to a

change of heart on the part of Hempel, who "at one time stated that universal laws are true [and] now contends that they need hold only approximately", and to the philosophers Scriven and Humphreys who "contend that most laws that are formulated by scientists are only approximated by most (if not all) empirical cases".

Consideration of some of the simplest methods of statistical summary might also suggest that statements which are literally false may yet be held to epitomise the truth. Take a group of (n) G -objects (where $n > 2$), each of which possesses a quality θ to some measurable degree but not all of which possess it to the same degree. Then, to abstract or simplify:

AB.1: we might employ the arithmetic mean of the θ of the group, saying that

$$(i) \text{ any } G \text{ has } \frac{\sum (\theta \text{ of } G_1 \dots \theta \text{ of } G_n)}{n} \text{ of } \theta$$

or again,

$$(ii) \text{ the representative } G \text{ has } \frac{\sum (\theta \text{ of } G_1 \dots \theta \text{ of } G_n)}{n} \text{ of } \theta$$

But, except by a fluke, it will be false of each particular G that it has just this degree of θ ; so (i) will clearly be false. Further, "the representative G " in (ii) will be a theoretical construct that has no single direct counterpart in reality; and some logicians might perhaps then want to claim that (ii) is false too, analysing it in such a way that it makes an existential claim that is false (just as they might interpret claims about the (merely hypothetical) G with perfect or infinite θ as false).

AB.2: to overcome this difficulty, we might take the median G with respect to θ , aiming to select one of the real objects as central. Then, in summarising G 's possession of θ , we might claim that

(iii) the median G (where G is ranked with respect to θ) has x units of θ .

(iii) will certainly be true where (n) happens to be an odd number. On the other hand, it is open to an interpretation on which it would be false if (n) is even; for by the usual conventions (the arithmetic mean of the two most central observations being taken) the median G would then be another entity merely theoretically invoked, and the problems of AB.1 would be reintroduced.

AB.3: or suppose that, as it happens, all but one of G have x units of θ . In this special case, it might seem natural to take the mode as a summary measure, saying that

(iv) modal G 's have x units of θ

or perhaps that

(v) in general, G 's have x units of θ .

(iv) will be true. But (v), which might be taken to mean only that most G 's possess just this degree of θ , but might instead be held to mean that all G 's do, could again be false.

Thus both attempts, as it were, to extrapolate from reality (invoking limits or idealisations) and ones simply to summarise it in various standard ways do indeed seem sometimes to yield statements that could be counted as false. (Must they be counted as false? -Mayn't there be

a category of the "non-realistic", for which no existential claims are made? But that they "could be" could be enough for Friedman's case). When processes of abstraction are viewed in this light, Friedman may be right to suggest that insistence on stating only what would be sure to pass as strict truth may carry with it severe, and perhaps unnecessary, restriction of methods of description.

However, where what might be counted as falsehoods can fulfil the role of summarising detailed truth satisfactorily (for a particular purpose), there will of course be purposes for which they are not satisfactory (see Chapters 7 and 9). Furthermore, there seems to be a sense in which the truth that underlies them could be "recovered", if at a cost. Is the cost then only one of convenience? And, even for convenience, Friedman himself leads one to wonder how far, and how inevitably, this must be lost, seeming to leave open the possibility that eventually assumptions that are simple, powerful and true might be developed (see Chapter 4, note 14). Boland has claimed that a Popperian view suggests simplicity and "generality" could go together (see Chapter 4, note 10). And in his 1937 paper on "The Nature of the Firm", Coase seems similarly sanguine about simplicity in relation to "realism": quoting Joan Robinson's view that "the two questions to be asked of a set of assumptions in economics are: Are they tractable? and: Do they correspond with the real world?", he goes on to remark himself that "there may well be branches of theory where assumptions may be both manageable and realistic." But Coase also notes Joan Robinson's own view that "more often one theory will prove manageable

and another realistic". Just how much is there (or, will there be) to Oscar Wilde's quip that "truth is never pure, and rarely simple"?

Appendix CAs Ifs

Consider the statement:

(a) "Rupert is walking as if he is drunk."

What does (a) tell us? That if Rupert were drunk he would walk thus and thus, and that he's walking that way now. Or, to put this in schematic terms, that

(b) If K, then W ϕ -ly; and W ϕ -ly in fact.

It seems to me that, where (a) was uttered, both speaker and hearer would often presuppose or take it for granted that ^{when} Rupert is under the influence, this influences his way of walking, and that they are agreed on what the influence on his way of walking is. In these circumstances, to assert (a) might seem to be to convey simply the information that

(c) Rupert is walking thus and thus.

i.e. W ϕ -ly

When (c) would be a full statement of what the utterer of (a) intends to convey, I shall say that he is putting "as if" to a D-use (a descriptive use). (A different example where an "as if" seems inevitably to be used in a D-use: "Businessmen behave as if they're steamrollers"). Making a D-use of "as if" in (a), then, the speaker would be wanting to convey merely the bare fact that

(d) Rupert is walking drunkenly

But there seem to be other possible D-uses of "as if" for which no ready replacement of the "as if" clause would be available - where there is no other convenient, and perhaps no other accurate, way of specifying the content of "thus and thus"

(some examples: "Now, children, we're all moving as if we're pussy-cats after a mouse"; "It accelerated as if it were a Cortina saloon"). (This might have some importance for questions raised in Appendix B).

However, whereas, when a D-use of "as if" in (a) is made, the speaker's point is simply one about how Rupert is walking, there could be other utterances of (a) in which, although the connection between K and W ϕ -ly is not in doubt, another matter, besides Rupert's manner of walking, is: namely, whether Rupert is drunk. Then, since K is agreed to be a sufficient condition for W ϕ -ly, W ϕ -ly might be treated as evidence with a potential bearing on whether K (Rupert is walking thus and thus; what is causing him so to walk? - it could be his being drunk). Thus in (b), W ϕ -ly might be treated as evidence whilst K specifies the conclusion to which the evidence could be taken to point. When the utterer of (a) intends his utterance to be construed in this way, I shall say that he is putting "as if" to an E-use (an evidential use). (An example, where the "as if" seems inevitably to be used in an E-use: "He talks as if he knows his job").

But E-users of "as if" in (a) might believe that
 (E1) Rupert is drunk (and his walking drunkenly is evidence of this)
 or (E2) Rupert is not drunk (and though his walking drunkenly might be interpreted as evidence that he is drunk, it isn't actually evidence of this)
 or (E3) Rupert may be drunk (and his walking drunkenly may be

evidence that he is)

or (E4)Rupert is not drunk, but is merry (and his walking drunkenly is evidence only that he is merry)

The moral of this tale is that, from the written words "Rupert is walking as if he is drunk" alone, it will not be possible to tell whether the writer is making a D, E1, E2, E3 or E4 use of "as if"; and so to know whether or not he means to suggest that Rupert is drunk.

And if the writer has already said elsewhere: "of course Rupert isn't drunk" - or even "of course Rupert is only merry" then at least we know - if only we notice in time - that his use cannot be an E1 or E3 one. He is not suggesting that the "as if" clause is, or even may be, fulfilled. See Chapter 5.

Appendix DOn "Rational Economic Man"

This Appendix might perhaps have been called instead "Stop Press"; for Hollis and Nell's book Rational Economic Man (1975) appeared too late to feature in the text, yet is news too important to be left out of account. Between the structures of the argument in this thesis and in Rational Economic Man (REM) there appear to be some striking parallels: first impressions of the similarities and the differences are reported below.

Whereas I have, for the most part, been following Emmet and MacIntyre's problem-oriented approach, Hollis and Nell write (p.240): "our critique of neo-Classicism rests not on our critique of Positivism, but on our alternative to it, Rationalism". They "dispute not only the Positivist doctrines behind orthodox methodology but also empiricism in general", saying "it seems to us to be as true as ever that a scientific method must reflect a philosophy of science, which must reflect a theory of knowledge"(p.3). But it seems to me that the difference between the approaches may prove in the main to be one of strategy: their theory of knowledge is, I think, implicit in Chapter 8 above (which draws substantially on an earlier paper of Hollis(1967-8)); and the problems I tackle are also tackled in REM. The difference in strategy is very evident in the first part of each (where their critique is of Positivism and mine of Friedman's case for a particular hypothesis), but considerably less so in the remainder; and below, attention is confined to the similarities

and contrasts where explicit overlap is greater (Chapters 6 to 9 above with the latter half of REM).

The dilemma posed above in Chapter 6 in essence coincides, I think, with that posed in REM, p.240: "either economic agents and activities are conceived in such a way that the neo-Classical assumptions are sufficient to entail the vision of optimality...in which case the model cannot, in principle, apply to a world in which our present laws of physics and engineering hold: or economic agents and activities are conceived in a manner consistent with regular reproducibility, in which case the model can apply, but adulteration in the product and exploitation in the factor market are both conceivable, even likely, in equilibrium, optimality is a farce, and the door is open wide in welcome to both Veblen and Marx." Or again (REM, p.233): "If the [neo-Classical] assumptions are made at all realistically, then they generate ...objections which suffice to show that neo-Classical optimality cannot be guaranteed, indeed, could be achieved at best by accident. If the assumptions are not realistic they conflict with necessary properties of the bearers; and so the model will not apply at all."

On one horn of the dilemma: Hollis and Nell's point (REM, p.231) that when imperfections are recognised and the requisite additions are then made to the neoclassical model, "the basic model would...have changed beyond recognition" corresponds to the "curate's egg" charge developed in Chapter 7 above. However, they do not introduce the theory of the second best.

Taking the other horn: both REM and the thesis argue that neoclassicism founders because it "involves abstraction which ignores essential features"(REM,p.238) of the economy (the former concentrating more than the latter specifically on the essential features of production). But whereas I claim in Chapter 8 that the neoclassical (GE) world is an impossible one, Hollis and Nell make the apparently very different claim that "neo-Classical theory is inapplicable, not false, and impracticable, not impossible"(p.223). Yet the arguments on which these respective claims are based seem, at first blush, strikingly similar: is my conclusion over-strong, or might it be that theirs could be strengthened?

Again at first blush, some expressions of the REM challenge to neoclassical doctrine might seem strong enough to "tot up" to a charge of impossibility after all. Examples are: "Neo-Classicism finally falls foul of necessary but not purely logical truths"(p.241); "neo-Classical theories, being exclusively concerned with action variables, presuppose a model of economic agency, with which they are incompatible" (p.225): "we have argued not only that [neoclassical] assumptions are not fulfilled, but also that they cannot be, given the world we live in"(p.233); "we contend that there cannot be one-commodity worlds"(p.248).

But Hollis and Nell only "press for a conviction on charges of impracticability"(p.228), where "if a variable is so defined that an economic agent, to whom it applied, could not last long in the market (given the assumed social order) or support himself materially (given the laws of nature), then the model

in question is impracticable"(p.224). This suggests there might be two reasons for holding that the neoclassical "model", though impracticable, might yet be possible: namely, that

(a)the relevant agents could exist in the market (even though they couldn't last)
&/or(b)though such agents couldn't exist/last, given our present world, it doesn't follow from this that they couldn't exist/last under any conditions.

Hollis and Nell do not always seem to believe that (a) is true (saying, for instance, that "given the laws of physics and engineering and given the legal foundation of capitalism, there cannot exist neo-Classical bearers"(p.225)). Again, mightn't existence imply lasting? - or does lasting mean lasting for longer than that? And it seems to me possible to argue that the relevant agents couldn't exist in our world. What about (b)? The argument in Chapter 8 above was that there are limits on what can count for us as "possible worlds", and that the neoclassical "world", with its excessive uniformity, violates them (and thus that, not only is none of the relevant agents able to exist, given our world; but also that, given us, they couldn't exist in any worlds counting for us as "possible"). Is this argument too weak for the impossibility conclusion I draw, because even if something isn't possible for us (possible for men, as against nem), still it might be possible? But even if this suggestion makes sense (and it may not do), mightn't it still be argued that the neoclassical model presupposes that its "world" is populated by men (albeit men who are assumed - impossibly - to behave as nem)? And in any case, the neoclassical world

would still be not just impracticable-for but also impossible-for us.

Might it be that Hollis and Nell decided that arguments along the lines of my TATA were too abstruse to indulge in, in adequate detail, in REM? One passage rather suggests this: "to carry the argument a stage further...[connect] some 'laws of nature'...with the concept of a 'material object', arguing that this concept was itself a conceptual primitive for any understanding of an objective, experienced world. But this, although tempting, would be far beyond our scope". And then too, at the end of Chapter 8 of REM, they speak, surprisingly, of the impossibility charge as merely "so far unproven"(p.232).

But this is just a first report, dispatched in haste before a thorough investigation could be carried out on relations between REM and this thesis. Alas, there can be no later edition of the latter, giving fuller coverage.

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